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## SOME PHYSIOLOGICAL VARIATIONS OF PLANTS, AND THEIR GENERAL SIGNIFICANCE<sup>1</sup>

IN a survey of the domain of the biological sciences in recent years, one of the most significant facts is found in the extent to which physiology has invaded those fields of this domain which, in the earlier stages of development, seemed entirely apart from and independent of physiological relations. When species of plants were supposed to have been created at the beginning just as we find them to-day, and to transmit their original characters unchanged to their remotest possible descendants, there was no physiological question as to the variations within species, and none as to the relation of species to each other nor as to the origin of new species. In that view there could be no origin of new species. They were all created at the beginning, and then the Creator rested.

When botany first began to be a science it was merely an attempt to classify plants, that is, to discover the characters of species as they were originally created, to group together those that were most alike and to separate those that were unlike. The characters used in the first attempts at classification were more or less superficial, and systematic botany was merely a study in formal external morphology.

But a change has come; and this change began with the general acceptance, among biologists, of the view that species are not

<sup>1</sup> Presidential address delivered before the Michigan Academy of Science at Ann Arbor, Mich., March 28, 1907.



entities with *necessarily fixed characters*. Even though *some* species of plants have persisted with constant characters ever since their earliest records were inscribed upon the rocks, no biological theory has received more certain confirmation in recent work than the theory that species are even now in process of creation. The creative power is not resting, never has rested. Species are appearing before our eyes. We have only to open them and see. In short, nature has been caught in the act of originating new species.

I refer, of course, to the work of de Vries, who has found among the evening primroses species which, every year, are giving rise to forms among their offspring sufficiently different from their immediate parents to be regarded as elementary species. With some of these new forms the characters which distinguish them from their parents are constant when propagated by seed. This is not to be regarded as the inheritance of characters acquired by the parents of these new elementary species, but rather as the appearance of new characters in the race, not by a gradual modification of parental characters, but by a sudden transformation to which de Vries has given the name *mutation*. These new characters can not be ascribed to the direct influence of external factors on the adult or developing forms in which the new characters appear, since they appear and persist in the same conditions of life in which the parental type is continued. De Vries offers no explanation as to how these new characters are produced, but following his work, MacDougal has succeeded in producing new modifications by artificial means, using as the subject of his experiments species which are closely related to those with which de Vries obtained his notable results. MacDougal injected various substances, radium preparations, sugar

solutions, calcium nitrate and zinc sulphate, into the capsules of the plants experimented upon, before the eggs were fertilized by the nuclei from the pollen grain. From the many capsules used, a few furnished seeds which, on planting, produced plants notably different from the type of the parent plant. The flowers of the new type were closely guarded to prevent cross fertilization, and their seeds when planted gave a few plants which conformed in every particular to the new type.

If there is no mistake about MacDougal's results, and I see no reason for supposing there is, at least one very important conclusion seems to be well founded, namely, that in an early stage of development of the plant egg, before it has been fertilized, it may be so profoundly modified that the adult plant resulting from it is decidedly different from what it would have been had the egg not been so modified, and the modifications thus produced are transmitted to the next generation through the seeds. Taking the results of both de Vries and MacDougal, we may conclude that the necessary modification of the egg is sometimes produced in nature, and may also be induced by means under the control of the experimenter.

There is one question which will probably be both affirmed and denied by different biologists for some time in the future. It is this: Are the new types which appear by sudden leaps to be considered new species or only varieties of the parent species. The debate on this question will be all the more acrid and prolonged because of the impossibility of giving a satisfactory definition of the term species.

One ought to ask pardon perhaps for quoting *authority* in science, but a high botanical authority has said that he believes no better definition of species has ever been invented than this: "A species



is a perennial succession of like individuals." An equally high botanical authority has said that a species is a judgment. And this also is true. Species and other categories of classification are more or less arbitrary distinctions, made for convenience in classification of our knowledge. Hence in a given case, the question whether two different forms are to be regarded as two different species or not, is in part a matter of individual judgment. If Darwin's view is correct, that new species may originate by the gradual accumulation of exceedingly minute differences, there could be no line of demarcation between species provided we could have all the transitional forms. Only where the transitional forms had disappeared, or the new forms had migrated to a new region, could we have sharp lines of distinction between species. Even in the case where the new form had migrated to a region not occupied by the old, the transitional forms would be disclosed on studying the species in all its range. Distinctions of species in such a case must necessarily be more or less a conventional matter. But if species originate by the sudden production of entirely new characters, that is by mutations, as de Vries believes, then there are no transitional forms connecting the new to the old. The condition in nature in this case would be similar to that in which there has been an extinction of the transitional forms between two different types derived from a common ancestor by gradual modifications. In either case there is room for individual judgment in the delimitation of species, according as the differences between the two types are greater or smaller. We say, "A species is a perennial succession of like individuals." But how nearly alike must they be? No two individuals are exactly alike, and the extreme differences possible between two individuals of the same species

may be greater than those between two individuals of different species. In other words, the differences within the species may be greater than the differences between species, as de Vries has pointed out. How then are we to decide whether two individuals comparatively different from each other, and yet alike, belong to the same or different species? It has been found that for any given character the variations within the species may be expressed numerically by an average with deviations, both above and below that average. For instance, the average height of the stem in a given species of plants may be two feet. Most of the plants composing the species may vary only slightly from this average, say from one to three feet. But the greater the number of individuals examined and measured the more certain it becomes that we shall find a few individuals which differ far more widely from the average. In our supposed case we might find that the extreme limits of size were six inches to ten feet, while the average was only two feet. These deviations from the average of the species are called the fluctuating variations. They are largely determined by the external conditions in which the species grows. Professor George Klebs has shown that when plants are subjected to extremes of variation in the external conditions of light, heat, moisture and food supply, the deviations from the average of the fluctuating variations become far greater than are usually found in a state of nature. Klebs's results with *Sempervivum* were truly remarkable. He produced variations that are not found in a state of nature in the species with which he worked, changes in the color, size and shape of the flower, great variations in length of the stem and its mode of branching, the size, shape and arrangement of the leaves. As the result



of this kind of work, carried on for a considerable number of years, Klebs has given us a definition of a species which expresses the dependence of the form of the plant upon the environment. According to Klebs we must say: "To a species belong all individuals which, propagated vegetatively or by self-fertilization, under like external conditions, show the same characters through many generations." If two plants under these conditions show a noticeable difference, they are to be regarded as belonging to two species, even though they have descended from a common ancestor. Gaston Bonnier has shown by experiment that plants transplanted from the region of Fontainebleau near Paris to Toulon in the Mediterranean region show in a few years adaptations both of external form and internal structure which cause them to resemble the species characteristic for the Mediterranean region. The same investigator found similar results on transplanting from the plains to Alpine regions. Knowing the origin of such widely variant forms we do not call them two species, but merely extremes in the fluctuating variations of the species. It is conceivable, however, that nature might perform this same experiment on such a scale and in such ways as to make it difficult or impossible to recognize the common origin of two such different types. In that case the botanical collector or systematist, finding the two types in widely separated regions, would describe them as two species of plants. If the distribution of the species was continuous from one of these extreme regions to the other the connecting intermediate forms would show that we had to do merely with extreme fluctuating variations brought about by extremes in soil, moisture, heat and light. If, however, the geographical continuity of the species had been interrupted in any

way, it would be impossible to determine by observation alone that the two extreme types were only fluctuating variations of one species. That could be determined by the experimental method as followed by Klebs and Bonnier. Plant the two types in the same region, grow them under exactly the same conditions, and if after many generations they continue to exhibit constant differences they are to be regarded as two species. On the contrary, if they show the same characters under the same conditions, they are one species. Such a method of determining whether one has a new species or not involves an enormous amount of labor, and a great deal of time. It is not in favor with the systematists who work with the higher plants. Nevertheless, there is an increasing recognition among botanists of the necessity of physiological work even in those fields of research that have in the past been dominated by morphology alone.

Such experiments might help to decide the question whether the so-called alpine species have been constant since the glacial period, as de Vries supposes they must have been, or whether, as seems possible, similar combinations of climatic conditions, operating in widely separated regions such as the alpine region of central Europe and the high latitude of Norway, have produced species of similar form. It does not even seem necessary to assume that the parent species of the alpine forms has been the same in these widely separated regions. De Vries has pointed out that species sometimes overlap by what he calls *transgression variations*. Klebs has shown that in one species of *Sempervivum* he could produce nearly all the characters found in the other species of the genus. Is it not therefore possible that the continuation of conditions of soil, temperature, moisture and light characteristic of the alpine re-



gions, could produce a type varying about a new average, which lies near one of the extremes of the fluctuating variations of the parent species.

If this new average should be established within the limits of the transgression variations of two species one of which existed in northern Europe and the other in central Europe, we should have the production of similar types, the alpine and arctic type, in widely separated regions and from different parent species. The characters of the new type are not 'fixed' in the sense of being due to inheritance, but only in the sense that they are a response to a particular combination of external factors, and this combination is constant in the given regions. Such a view of the origin of alpine types is not merely of theoretical interest, since the application of the physiological method gives the means of reaching more or less definite conclusions.

De Vries and others have pointed out that the species of the manuals and the systematic botanists are in large part composite or collective species and not simple or elementary species. In his view the latter differ from their parent species by *new* characters, not by modifications of old ones. The new characters are inheritable as soon as they appear, and are not regulated by the external conditions in which the adult plant lives.

If MacDougal's work stands the test of repetition, physiological experiment may open up a new field in investigating the *origin* of species. One method of applying physiological experiment to determining the *limits* of species has just been discussed. But other applications of this method are possible. It is well known that cross fertilization generally takes place only between closely related species of plants, rarely between genera. When attempts are made to cross species remotely

related, either the pollen does not grow upon the stigma of the strange species, or fertilization of the egg does not take place, or if seeds develop the resulting hybrid is sterile, not being able to produce seeds for its propagation. What lies at the basis of these physiological differences is still obscure. It is probable that enzymes, toxins or other chemical substances play a part. But whatever the explanation, the fact may be used in determining the nearness or remoteness of the relationship between forms. This possibility has been recognized by many investigators, and biologists have proposed using the degree of fertility of hybrids as the means of distinguishing genera, species, and varieties. Though this has been found not to be reliable in all cases, de Vries has suggested it as a means of distinguishing his elementary species from varieties. If on crossing two forms the resulting hybrid is constant in regard to a given character, when guarded against further crossing, the two forms were different species. But if, on crossing, the descendants of the resulting hybrid followed Mendel's law of hybrids, according to which one fourth of the offspring of the hybrid in each succeeding generation resembled one parent in respect to a given character, one fourth resembles the other parent as regards the corresponding character, while half are like the original hybrid, then the parent forms of the hybrid were one and the same species.

Whatever the limitations of this method in its practical application, the significant fact is the extent to which physiological conceptions have invaded a realm that was purely morphology. We may use the experimental method in studying the origin of new species and varieties. We may apply physiological methods in determining the range of the fluctuating variations within the species. We may use physiological affinities as the test of the degree



of relationship existing between different forms found in nature.

The foregoing discussion has had special reference to the higher plants. But among the lower forms of plant life physiological methods are far more applicable, indeed necessary, in determining the characteristics of species. In all that group of plants known as bacteria, species can be distinguished only by physiological means. These organisms are so simple in structure, their morphological characters are so few, it is utterly impossible to classify our knowledge of them even from a systematic point of view without using physiological means as the basis of species distinctions. The most important relations which the bacteria bear to the organic world in general, and to the human race in particular, are physiological in their nature. Some of them have the power of invading the animal body and producing there substances which we call toxins, and which may be so exceedingly poisonous that the result may be fatal in an extraordinarily short time. Fortunately the animal body has the power to vary its ordinary physiological processes in such a way as to produce antitoxins which neutralize the action of the toxins. A given organism may vary in its virulence at different times. An epidemic due to an organism in the so-called attenuated state, produces a mild form of the disease. A given animal or plant may be especially resistant to the toxin of one species of bacteria, as the horse is to diphtheria toxin, or it may be very susceptible to a given toxin, as the human body is to the toxin of tetanus, or lockjaw. Also the same organism shows different powers of resistance, or immunity, at different periods. It is well known that any conditions of life that produce a low state of vitality in a given individual, make that individual far more susceptible to disease, that is, to the toxins of other organisms. Not only are plants

and animals susceptible to the toxins produced by other plants and animals, but each organism produces substances which are toxic to itself. This is true not only for the lower organisms, but at the present time a discussion is being carried on as to whether the necessity of the so-called rotation of crops of higher plants is more dependent upon the partial exhaustion of the soil in elements necessary for a given crop, or upon the gradual accumulation in the soil of substances detrimental to the kind of plants that produced them. The physiological variations of organism, and the physiological relations of one kind of organism to another, form a series of the most fascinating as well as the most difficult of biological problems. The small size of the bacteria and the rapidity with which they multiply make them very favorable subjects for experiment along the line of the fundamental biological processes. An organism that requires several hundred years to complete its life cycle is obviously not a favorable subject for an experiment that requires the study of several generations. But if, as in the case of some of the bacteria, a new generation may be produced every fifteen minutes, it is possible to obtain within a few hours hundreds of generations and millions of individuals.

There is another group of organisms about which I wish to speak, not so simple as the bacteria in structure, but far inferior in that respect to the highest plants. I refer to the filamentous fungi, and I wish to call your attention to some facts that again have to do with the question: What is a species? As in the case of higher plants, the first attempts to classify these organisms were upon a purely morphological or structural basis. But a deeper knowledge of their life histories and physiological variations makes it more and more apparent that here, as among the bacteria,



it is necessary to use physiological means of distinguishing, shall we say species? For the present we can avoid making the decision, and say forms or races, yet at the same time we can hold our minds open to evidence as to whether these forms or races are not, after all, incipient species. Two groups of these fungi especially force themselves upon our attention from the point of view we are considering. One of the groups has been called the Uredineæ or rust fungi, and the ordinary rust of cultivated cereals is a typical example. The other group is known commonly as the mildews, or more technically the family Erisiphaceæ. The rose mildew and the grape mildew are common examples. In both of these groups it has been found necessary to distinguish what have been called biologic forms or races, distinguished from each other only by the fact that they differ in capacity to infect different species or genera of the host plant. Working with the wheat rust, which was formerly supposed to be the same on any of the cultivated cereals and wild grasses, Eriksson has found that there are numerous races adapted more or less closely to the species of single genera, and they are able to infect species of other genera either with difficulty or not at all. Their forms can not be distinguished morphologically, and yet the infection experiments show that physiologically they are decidedly different from each other. In trying to conceive the origin of these forms, there seem to be three possibilities. First, these biologic forms may have had an origin from different species growing on a narrowly limited group of host plants. There seems to be little evidence for this view. Second, they may have been derived from one species, by sudden physiological changes in the fungus alone, without any influence of the host. This would be similar to the origin of elementary species by mutation,

as found by de Vries among the evening primroses. There seems to be no direct evidence for this view. Third, a group of biological forms which can not be distinguished morphologically may have originated from one species which at first grew on a wide range of host plants, but when a strain or race is propagated continuously on the same species of host, there is a special adaptation of the fungus to that species of host, and it becomes able to infect that one more readily, and others less readily, and at last not at all. For this view there is some direct evidence. A form of rust which was capable of growing on four genera of host plants, was propagated for ten years continuously on only one of the four. At the end of the ten-year period it could infect that one genus strongly and the other three weakly or with uncertainty. If this experiment indicates the way in which the biological forms have come into existence, they have originated, not by mutation, but by adaptation. The differences they exhibit have come about by the gradual accumulation of imperceptible modifications.

Among the mildews there has been found an adaptation of forms even closer than among the rusts. Experiments of Salmon on the mildews of grasses disclosed the fact that adaptation is not only to one or few genera, but in many cases actually to one or a few species within the genus. The mildews exhibit the phenomena of adaptation carried much farther than it is carried among the rusts.

The question remains, can these biologic forms or adaptive races ever rise to the dignity of true species? Again the direct evidence is lacking. But if these fungi are as variable in their morphological character as Klebs found even the flowering plants to be under different physiological conditions, we might expect the same causes which bring about the physiological



adaption to be able to produce morphological differences as well. But even if no morphological differences appear, are we not justified in making physiological characters the basis of species among the fungi as is already done among the bacteria? The speaker is inclined to answer this question in the affirmative. It seems certain that, for practical purposes at least, it is becoming absolutely necessary in other groups of fungi, as well as in the rusts and mildews, to make distinctions on physiological grounds, not to the exclusion of morphology, but in addition to it. Whether you call the groups of individuals so distinguished species or not, matters very little. The important thing is that the distinction must be made.

It is impossible to apply de Vries's test for species and varieties among the fungi. For most of them there can be no such thing as cross-fertilization. For many there is no fertilization at all, and even where present, it is generally strictly self-fertilization. Naegeli long ago pointed out that where plants were propagated only vegetatively or by self-fertilization and it may be added parthenogenetically, individual peculiarities were perpetuated in the descendants, while with open or cross-fertilization the peculiarities of one individual may be modified in the next generation by mingling with another line of inheritance representing peculiarities of another individual opposed to those of the first. Open or cross-fertilization therefore tends to keep the species homogeneous by neutralizing extreme individual variations. While in those plants which are propagated by parthenogenesis, that is where the eggs develop without fertilization, or by self-fertilization, or by non-sexual spores, or by vegetative means, the species tend to become heterogeneous. They are made up of many lines of descent which are never mingled, individual peculiari-

ties tend to become extreme, and species limits are particularly difficult to determine. Among flowering plants the hawk-weeds furnish an example of the results of reproduction by parthenogenesis. In this genus, *Hieracium*, it is said that of two noted men who had made a special study of the species of this genus, neither could identify the species by the other's descriptions. The same result is apparent among the fungi, in the development of the biologic forms or adaptive races.

Individual adaptation to a given host is not neutralized by fertilization from a plant with a different adaptation, but is continually accentuated. The practical importance of many of these adapted forms compels us to recognize them as distinct entities, and to give them names. For practical purposes then they are species, even though they can be distinguished only physiologically.

This capacity for physiological variation or adaptation on the part of fungi is significant in another direction. It is certain that among the fungi as well as among the bacteria, forms that for the most part live only on dead organic matter, that is, as saprophytes, may under certain special conditions become adapted to a parasitic life. They thus become the producers of new diseases. Though for the most part supposed new diseases are only a wider distribution of old diseases, it is entirely possible for new diseases actually to originate by physiological adaptation. This has been proved in the production of plant disease experimentally.

But if this kind of variation has its somber side, there is also an obverse side. Physiological variation enables us in many cases to select and propagate cultivated plants that are particularly resistant, and sometimes completely immune, to a given disease. The same phenomenon may be observed here as in the human family. In



any given epidemic there are always certain individuals who never contract the disease. They have a certain natural immunity to that particular disease, and this immunity is due to some physiological peculiarity. So in a field of rusted or mildewed wheat some individual plants show themselves more resistant than their fellows to the species of rust fungus found upon that species of host. By selecting and propagating these immune individuals we may develop an immune race or strain. The problem is not always so simple as here stated. It may happen that a race immune to one disease may be very susceptible to another, or immunity may be accompanied by other qualities altogether undesirable. One might be led to suppose, on reading certain popular articles intended to show how new forms of plants are produced, that it is only necessary to imagine an ideal plant and then set to work to create it. Nothing is farther from the truth than this. Nature does sometimes produce something new, as a stoneless plum, or a nectarine on a peach tree. But man must take the materials furnished by nature, combine them in new ways, or modify them within limits which are usually soon reached. He can not create a wheat plant immune to rust, nor a watermelon resistant to the wilt fungus. But if nature furnishes a few individuals with the desired qualities, man can propagate the individuals possessing those qualities, and by rigid selection maintain the qualities to a high degree. If it is possible to cross the plants with other species or with varieties of the same species, he may be able to combine in the same individual a number of desirable qualities. Having obtained these qualities in one individual, he can best conserve them by vegetative propagation, such as by grafts, cuttings, bulbs or tubers, according to the habit of the plant propagated. He may care nothing

whatever about the limits of species or varieties except in so far as their physiological relations help or hinder his combinations. Following MacDougal's method, it may be possible to produce in plants some new characters. But even if it were possible to produce in this way really new species, it is hardly within the range of possibility that we could choose beforehand the kind of a species we would produce. It would be a case of 'cut and try.' If the result be a form with desirable qualities, let it be preserved, but if it be worthless, let it die. Nature has repeated this experiment ten thousand times. If we would imitate her we must search out her secrets in the physiological realm. She conceals them well, but is not unwilling to reveal them to him who questions her with a hearing ear, a seeing eye, and a thinking brain, tools which she herself has given him.

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*A PLEA FOR THE STUDY OF THE HISTORY  
OF MEDICINE AND NATURAL SCIENCES<sup>1</sup>*

FOR a number of years a new current of thought has been gradually coming to the front in the minds of scientific thinkers of the times. The nineteenth century, the mental development of which is now assured, has of late been severely criticized for its unhistorical character, and perhaps not without reason. Over this inheritance from the preceding generation a certain dissatisfaction is being more and more keenly felt in the most diverse branches of science. The main trend of the last century was naturalistic and economic to a marked degree; so much so, that the new methods discovered in natural science, and

<sup>1</sup> Read before the American Anthropological Association, at the meeting of the American Association for the Advancement of Science, December 31, 1906.



the vast progress resulting therefrom, seemed to foreshadow an entirely unprecedented epoch in the history of science, and the generation of that age was only too eager to sever all links connecting it with the accomplishments of former ages.

The inauguration of the twentieth century presents a somewhat contrary aspect. One of its primary tendencies has been towards a restoration of our lost connection with the eighteenth century and with earlier periods, resulting in a movement of such earnest and impressive character that we can not foretell at the present moment whether the eighteenth century will not, at some day not far off, seem nearer to us than the sober prose of the nineteenth.

It is not mere chance that at the dawn of the new age the war-cry 'Historical investigations!' is sounded from all camps, and that in consequence a broader scientific knowledge is obtained through this pursuit of historical research. In nearly all lines, students had become weary of the worn and time-honored ruts, and from the dry atmosphere of specialized specializations yearned for the purer air of loftier heights; and not least among the causes of this reaction was the disappointment due to the misapplications and failures of the evolutionary theory. New ideals were thus created, and found their expression in an extended historical movement, which led to radical changes and to amplifications in literary activity, in academic instruction, and in museum policy—or rather in encouraging prognostics of a new museum era—at least, so far as Germany, Austria and Switzerland are concerned. To give a concise idea of what has been accomplished, and is being proposed to be done in this line, is the object of this paper.

To review even hastily all literary pursuits pertaining to this large field is naturally beyond the scope of my purpose.

The most noteworthy, in my estimation, are the following: the journal *Zoologische Annalen*, founded in the interests of the history of zoology in 1904 by Max Braun, professor of zoology at the University of Königsberg, and the organization of the Deutsche Gesellschaft für Geschichte der Medizin und der Naturwissenschaften, in Hamburg, on September 25, 1901,—a most active and industrious society, which, now under the able leadership of Professor Karl Sudhoff, of Leipzig, has thus far published six volumes of 'Mitteilungen zur Geschichte der Medizin und Naturwissenschaften.' The pages of this journal are full of interesting original contributions and copious reviews concerning the history of anthropology, botany, zoology, geography, geology, mineralogy, chemistry, physics, mathematics, astronomy, technics, and medicine. A distinguished production of German scholarship is the 'Handbuch der Geschichte der Medizin,' established by Theodor Puschmann, the late celebrated medico-historian of Vienna, and edited by Max Neuburger and Julius Pagel. It was recently completed in three volumes, with thirty-one contributors, and embraces the history of medicine in all its departments and epochs, among all peoples of the globe, inclusive of primitive tribes. Despite its very numerous shortcomings—chiefly due to inaccessibility or want of material, especially on Asiatic medicine, but partially also to lack of historical criticism—it remains, nevertheless, a remarkable monument, but more prospective than retrospective. The recent proposed action of the Berlin Academy of Sciences in regard to the publishing of a complete edition of the Greek medical authors may also be mentioned in this connection; and the new epoch-making researches on the life, personality and works of Theophrastus Paracelsus.



In the academic institutions of Germany and Austria, broad and liberal space is now allotted to instruction and research-work in the history of medicine, natural sciences, and particularly in that of cultural plants and domestic animals. It is an officially acknowledged, fostered and encouraged subject of teaching and study; and there is now hardly any German university, however small, where it would not find a competent representative. Only a year ago (1905) an institute for the study of the history of medicine, in connection with a full professorship, was established at the University of Leipzig, the chair being occupied by Professor Sudhoff, who tells me that thus far there has been an average attendance of from fifteen to twenty-five students in his courses. This institute will regularly issue scientific publications from the beginning of next year (1907).

Berlin has two professors for the history of medicine—Pagel and Schweninger—it having been customary for many years for students of medicine to be allowed to choose their theses from this field, which has been done by many of them with evident success. Regular courses are offered there, besides, in the history of epidemic diseases, of anatomy, of chemistry, of astronomy, of cultural plants. In the last-named subject, four courses are tabulated this winter—one of general character, and three special ones relating to the cultural plants of Africa, and to those of the German colonies and the tropics, respectively. The University of Vienna has likewise two representatives of medical history (Neuburger and v. Töply), general courses and a special course on the history of physiology (Kreidl). Innsbruck possesses a specialist in the history of zoology (v. Dalla-Torre). An *extraordinariat* for the history of medicine has been founded at Würzburg (Helfreich); and courses on the subject are pro-

vided for at Bonn, Göttingen, Breslau, Heidelberg, Tübingen, Munich, Marburg, Kiel, Rostock (with even three teachers), further at Graz in Austria; and at Basel, Zürich and Bern in Switzerland.

On November 13, 1906, the cornerstone of the German Museum of Masterpieces of Natural Science and Technics, in Munich, was laid,—the last creation born from this young historical spirit. A question much ventilated now, in the circles of Germany interested, is the plan of a comprehensive museum for the history of medicine, illustrating its development, from the times of prehistoric man down to the present day, in anatomy, surgery, hygiene, endemic diseases and other phases.<sup>2</sup> Such a medical museum, fully deserving of the name, as yet exists nowhere. The medical faculty of the University of Paris moved a resolution to this effect some years ago, but the scheme has not yet been carried out. The only institution that has thus far made any attempt in this direction is the Germanic Museum of Nürnberg, whose very beautiful collections, however, are restricted rather to pharmaceutical than to purely medical antiquities. The first temporary exhibition relating to medical history was held in Düsseldorf in 1898, on the occasion of the annual assembly of the German naturalists and physicians; and similar tendencies developed at the Russian congress of physicians at Moscow in 1900, with greatest success.<sup>3</sup>

<sup>2</sup> See a paper by Sudhoff, 'Zur Grundsteinlegung des Deutschen Museums von Meisterwerken der Naturwissenschaft und Technik,' *Begrüssende Gedanken und Ausblicke* (reprint from *Münchener Medizinische Wochenschrift*, No. 46, 1906).

<sup>3</sup> Compare report on address by B. Reber, 'Über Notwendigkeit und Wert von Sammlungen betreffend die Geschichte der Medizin,' in report on 78. Versammlung Deutscher Naturforscher und Ärzte (reprint from *Münchener Medizinische Wochenschrift*, No. 47, 1906, p. 8).



I now venture to suggest that such a museum, representing the development of medicine, natural sciences and technics in their whole range, be established in this country, perhaps here in New York, which seems to be the most appropriate place for it; and I am under the strong impression that such an institution would be of wide and universal benefit to our public at large, and would contribute immensely towards the furtherance of science, both natural and historical, and also considerably aid the cause of anthropology. The temporary tuberculosis exhibit in this city last winter may serve as a technical example of what could be accomplished here. If a sufficient number of notable physicians of New York could be interested in the far more extensive plan just proposed—the carrying out of which would not require an exorbitant capital—its realization would seem to be within easy reach. Nothing would be more welcome to us than the sympathetic cooperation of physicians, to interest whom in the study of anthropology we must make many more and larger efforts, especially when we consider how signally anthropology, in its theoretical and practical bearings, has progressed and been advanced by medical men in Europe. One of the foremost tasks of the future American museum devoted to medical science would certainly be to represent the accomplishments of the hygiene, and technical inventions. In this way we should enlist the interest of physicians in our native population; and students of anthropology might also profit from their mode of viewing the subject or from an active participation in our work. A museum of this type, if developed on the broadest lines, may indeed lead also to new and fruitful anthropological work. I need hardly accentuate here the point that a full historical representation of all endemic and the great epidemic diseases (analogous to

the idea of the tuberculosis museums), in connection with the development of hygiene, would be a matter of great public service—an undertaking which should meet with the support of all philanthropists. It goes without saying that a museum of this kind would be a scientific, social and educational potency of the highest order—an agency of social progress, not inferior in rank to art or ethnographical museums.

At the same time I may be allowed to express the wish that the study of the history of medicine and the other natural sciences be taken up in this country with the same energy as on the other side of the Atlantic. I need not dwell here on a discussion of the manifold advantages of such pursuits, as the development of all science as an emanation of human culture naturally falls under the head of anthropology.

The most obvious gain which could be derived from the carrying out of these suggestions would be closer affiliation and more intimate contact of all the sciences. In the pursuit of historical investigations, we are all on common ground, and the character of the subject necessitates mutual dependence and assistance. It logically leads to a plea for cooperation, through the efficiency of which many of our most important problems are awaiting their final solution. Allow me to recall to you the study of the history of cultural plants and domestic animals, as constituting the framework of all higher forms of human culture. These topics have engaged the attention of anthropologists to a very limited extent only, being mainly worked up by botanists and zoologists, and occasionally by geographers and economists. The leading books on the subject are little satisfactory from the historical point of view, while historical investigations already in existence suffer from the lack of botanical or zoological accuracy. There is an unharmonious dissonance be-



tween these various attempts; and a syn-  
thetical representation that should seek to  
reconcile the conflicting standpoints is still  
a vain hope. The reason is the isolation  
of the single sciences, each of which, being  
restricted to its peculiar resources and  
methods, is intent on solving a problem in  
which a goodly number of them are in-  
volved. Naturally, only one solution to a  
problem is possible, whether it be attacked  
through physical or historical research;  
and if the results obtained by either are  
mutually contradictory, this is equivalent  
to saying that the particular science alone  
is unable to solve it, and that the solution  
should be undertaken by a concentration of  
energy of all the sciences concerned in the  
specific case. To cite a practical example,  
take the origin and propagation of our  
cereals, or the long history of the domestica-  
tion of the ox or the horse—problems  
around which, finally, the most ancient  
history of Asia and Europe centers.  
There is no science which, by the mere  
exercise of its own limited faculties, could  
reach a decisive solution of them; but I am  
fully confident of ultimate success through  
a cooperative combination of the various  
sciences involved, which, in this case, are  
geology, botany, zoology, archeology, his-  
tory and anthropology. The individual  
can not master all these sciences; and,  
instead of dividing our strength by working  
singly from isolated positions, we should  
advocate the uniting of all available forces  
for the best good of the same cause. The  
identical observation holds for all historical  
studies of sciences. The students of Ori-  
ental fields, for example, whether their  
work be in the Egyptian, Arabic, Indian  
or Chinese departments, are almost daily  
confronted with the wonderfully rich scien-  
tific lore of these peoples referring to sub-  
jects in which they themselves are not com-  
petent; but it is on the shoulders of these

very students that the accumulation of a  
large portion of the material rests, on which  
the historian of science can build. One of  
the most remarkable instances of this sort  
of cooperation which I have in mind, and  
which might be extended over many other  
lines, was the association of the Orientalist  
Karabacek in Vienna with the naturalist  
Johann Wiesner, for the investigation of  
ancient Arabic, Chinese and Turkestan rag-  
papers, the microscopical and chemical  
analysis of which confirmed step by step,  
in minutest details, every result of the his-  
tory of the invention of rag-paper con-  
tributed from Chinese and Arabic sources.  
The result of their joint labors, carried  
through many years, I consider one of  
the greatest triumphs of modern science.  
But there are many more culture problems  
of equal importance whose solution must  
be achieved in a similar manner. Let me  
refer you only to the history of the inven-  
tion of gunpowder and of the magnetic  
compass, both of which are still very ob-  
scure in fundamental points, and the work-  
ing-up of which requires a whole force of  
well-trained specialists—Arabists, San-  
skritists, Sinologues, and men well versed  
in chemistry, technology, physics and their  
history.

A study of some of the principal ques-  
tions in this field is further of profound  
significance in an interpretation of the  
methods and results of anthropology.  
Allow me to exemplify this briefly from  
the instance of mathematical history. The  
relation of the concepts of mathematics to  
the human mind and to the development  
of culture is still a matter of controversy,  
and one of burning actuality, just at the  
present time. A solution on the basis of  
an historical method is one of the aspects  
of this problem. The historical position of  
mathematics, however, is as yet very far  
from being defined, and no criterions are



agreed upon which will admit at the outset of stamping a mathematical thought and theorem as borrowed or independent. The most striking feature in the history of this science is the fact that the same results, even in the highest branches of it, have frequently been obtained by different peoples and at various epochs, with little or no possibility of pointing out an historical connection between such coincidences. The quadrature of the circle, for example, was made the object of correct speculation in China, even in pre-Christian times; or the rule of Horner 'for solving equations of all orders,' established in 1819, was known to the Chinese 520 years earlier, when, in an arithmetical treatise published in 1299, roots were extracted as high as the thirteenth power.<sup>4</sup> Paul Harzer,<sup>5</sup> astronomer at the University of Kiel, last year submitted the mathematical knowledge of ancient Japan to a careful and ingenious examination, and has arrived at the conclusion that the Japanese found spontaneously adequate evaluations of the ratio  $\pi$ , and made the independent discovery of the binomial theorem, which they utilized for obtaining important results. Modern criticism, with its aggressiveness towards the groundwork of human knowledge, towards even that which seems most secure, has recently attacked also the foundations of mathematics, generally looked upon as the most unobjectionable science, and has designated its results, like those of other sciences, as more or less conventional, not necessitated by the nature of the human mind.<sup>6</sup> To us, mathematics is essentially an outcome of human culture; and the question arising from an anthropological view-point

is, Are the phenomena of mathematical thoughts to be considered as on an equal footing with those of language, religion or medicine, and, accordingly, capable of methodical anthropological treatment, or are they the particular productions of individual thinkers, and, accordingly, conducive only to an exclusively historical analysis? It is impossible for the present to pronounce a verdict on this intricate problem, though I should like to say tentatively, and with all reserve, that the present state of our knowledge of the mathematics of India, China and Japan would almost seem rather to favor the acceptance of the former theory. At all events, the ventilation of this question is well illustrative of the paramount importance of the study of the history of mathematics and its principal bearings on our views of the intellectual history of man.

The practical proposition which I finally wish to lay before you is, that working committees, cooperative in character, be organized, each consisting of a limited number of members selected equally from students of natural sciences and students of anthropology, especially those in Oriental fields, and pursuing given problems *viribus unitis*. Each of these unions, which need not be of an official character, but may be freely private voluntary alliances of interested students, should be in charge of a particular branch of science. Altogether, seven may be necessary—one for the study of the history of mathematics and astronomy; others for that of cultural plants, domestic animals, physics, chemistry, technology and medicine. Each committee should be so constituted that the united forces of its laborers will represent a consummate systematic knowledge of the subject in question, and take up, suggest, encourage and elaborate pending problems by the concerted action of all its partici-

<sup>4</sup>A. Wylie, 'Jottings on the Science of the Chinese Arithmetic,' in his 'Chinese Researches' (Shanghai, 1897), pp. 163, 184, 185.

<sup>5</sup>Paul Harzer, 'Die exakten Wissenschaften im alten Japan,' Kiel, 1905.

<sup>6</sup>Harzer, *ibid.*, p. 26.



pants. By this method of research, much time and labor would be saved, and more positive and enduring results would be secured.

In concluding, let me call your attention also to the fact that we do not yet possess a history of anthropology, and that broad-minded contributions to the history of our science are an urgent necessity. Goethe has said somewhere that the history of science is the science itself; and I believe, further, that only by a correct appreciation of the development of our science are we able to be just towards our fellow-workers and ourselves. Now that so many of our prominent leaders, like Brinton, Powell, Cushing, Virchow, Bastian and Ratzel, have passed away, whatever we may personally think of the value of their work and its influence upon future generations, it is our duty to come to an objective understanding of their activity and aspirations, and to write the pragmatic history of anthropology in the life and labors of its most conspicuous representatives.

BERTHOLD LAUFER

#### SCIENTIFIC BOOKS

##### THE MISSION FOUREAU

*Documents Scientifiques de la Mission Saharienne, d'Alger au Congo par le Tchad.* Par F. FOUREAU, chef de la mission. II<sup>me</sup> fasc., Orographie, Hydrographie, Topographie, Botanique; III<sup>me</sup> fasc., Geologie, Petrographie, Paléontologie, Esquisse Ethnographique, notes sur la faune, Préhistorique, Aperçu Commercial, Conclusions économiques, Glossaire. Index. Atlas. Paris, Masson et Cie. 1905. 4to, 1210 pp., maps and ills.

While the Mediterranean and mid-African colonies of France have been for some time fairly well known, the efforts to connect them by a line of geographical exploration had been rendered ineffectual by the difficulties and dangers of the route through the desert, and the hostility of the natives. Several expedi-

tions met with disaster and were exterminated by the fanatical population.

Finally the expedition organized and carried out by Foureau in 1898 to 1900 met with success. This happy result had been well earned, because Foureau had already given twenty-three years to Saharan exploration under the auspices of the Ministry of Public Instruction. In 1898 his itineraries already amounted to 21,000 kilometers, of which more than 9,000 km. were in previously unexplored country.

In 1894, M. Renoust des Orgeries left to the Société de Géographie a considerable sum of money, to be devoted to the geographical development of the French colonies. Out of this legacy the society devoted 250,000 francs to the purpose of the Sahara Mission, a sum to which the government added not only funds but an escort of 250 picked soldiers under the command of a most competent African officer, Com. Lamy, who fell during an attack by an African chief, in the very moment when the success of the expedition was assured.

With the concurrence of men of science, the commander of the expedition has prepared this report, which by the assistance of government and various scientific societies, is now published in magnificent style by the Société de Géographie, with a preface by Alfred Grandidier.

Together with the reports indicated by our synopsis of the title, the work is replete with well-executed maps, sketches, plans of towns, views and everything which could be of use to future explorers, including minute notes as to the presence, amount and quality of water, pasturage, cultivated land, wild animals, etc. If one is startled by the frequent notation, along the river, of the presence of 'oyster banks,' hundreds of miles from the sea, reflection recalls the colonies of the fresh-water *Ætheria* to which these notes undoubtedly refer.

The mass of information in this encyclopedic work, it is, of course, impossible to summarize. A few notes may, however, have interest for the reader. While a large portion of the herbarium suffered from termites and the wreck of a canoe, nevertheless a good number of plants are recorded; and numerous



small photographs reproduced in half tones in the text give excellent ideas of the manner in which many of the trees and shrubs appear in their natural state. M. Bonnet has made the most of the botanical material which was saved, and the record of native names for the various plants is remarkably full.

The expedition started from Biskra and traveled almost due south about 1,260 geographical miles to the settlement of Zinder, in the Damergou country, when they diverged to the east until Lake Chad was reached, passing round it by the north and east, whence they followed up the river Chari in a southeasterly direction, crossing from its headwaters to the Obangwe, an affluent of the Congo, down which they proceeded to the sea. The rocks encountered in the northern Sahara were largely: first, cretaceous limestones of Cenomanian age; then Devonian and a little Silurian, more or less interspersed with crystalline schists. In the middle of the Sahara the schists and granites prevailed, mitigated by patches of volcanic character. To the southeast of Lake Chad, more volcanics were encountered, and, on the upper reaches of the Chari River the schists, granites and diorites again occupied the field. Excellent views of the rocky and sandy desert with its characteristic dunes abound in the text. The first discovery and recognition of Silurian rocks, with *Climacograptus*, appears to be due to this expedition. M. Emile Haug has carefully worked out the Paleozoic fossils as well as the remains of some Cretaceous vertebrates, Selachians, fishes and reptiles, including two new species of *Ceratodus*, referable to the Albian.

The native Tuaregs of the northern Sahara are well described by Foureau, who shows how their predatory habits naturally result from the state of semi-starvation in which they exist, and which leads them to travel even 700-800 kilometers to make a raid, of which the chief result for them at best can be but the securing of a few camels and a few full meals of meat. They have an alphabet and system of writing of their own, but very few of them, those chiefly among the women, know how to

use it. The correspondence and business of the most wealthy among them is done chiefly in Arabic, by Arab scribes from Touat, who act as secretaries. Those who can write are fond of inscribing the characters on rocks; and few are the suitable blocks of smooth stone in the vicinity of their trails which do not bear some inscription. They are great and fluent liars, but among themselves do not steal, and, within the tribe, observe the law of the cache. They are monogamists, and their women enjoy great liberty, and possess powerful influence which they seem not to abuse.

The Tuaregs dress in voluminous garments. They cover the whole body and head, and veil the lower part of the face, usually with a strip of blue cotton, so that the eyes alone are visible. This practise, perhaps originally adopted to shield the skin from the ardent reflections from the arid soil, has become almost a religious observance. The influx of slaves and wives from the negro races to the south, the Arab and Egyptian blood derived from the east, have made these people almost incredibly mixed in blood. Careless in their observation of the rules of the Koran, regarded almost as infidels by the orthodox Mohammedan, they are, when it is a question between them and strangers, fanatically bigoted Moslems.

M. Hanay has devoted much consideration to the prehistoric remains of the stone age collected by the expedition, many of which are illustrated by admirable heliotypes. In his summary the commander of the expedition pleads for the native people that they should be allowed to practise the rites of their religion in peace; and has very little to say in favor of the results accomplished by the French missionaries, in the regions to which the latter have had access.

On the whole the work is a mine of material for the geographer, geologist, naturalist and ethnologist, and reflects the highest appreciation of the labors of those who have succeeded in bringing so arduous an expedition to a happy and successful conclusion.

W. H. DALL



*The Crawfishes of the State of Pennsylvania.*

By ARNOLD E. ORTMANN, Ph.D. Memoirs of the Carnegie Museum, Vol. II., No. 10.

There are two groups of animals which seem destined, on account of their abundance, extensive North American range, plasticity and educational utility, to become classics in the literature of American zoology. One of these groups is the Garter Snakes (*Thamnophis*) and the other the Crawfishes (*Cambarus*). The importance and utility of these groups does not seem to be generally recognized. It is therefore of special importance that Ortmann's recent comprehensive treatment of the Pennsylvania species of crawfishes should be generally known.

In his introduction Ortmann mentions his extensive explorations of the state, in which he traveled over 11,000 miles, and discusses his methods of collecting and finally gives a historical summary of the Pennsylvania species. This section is followed by the taxonomical and chorological portion of the paper, which includes a key to the species, with detailed description of each, full records of the occurrence of each species in the state and elsewhere, and a critical summary of the geographic range of each species. The detailed character of these records makes the work of special value to students of other localities as well as those of Pennsylvania.

The section devoted to ecology and geographic distribution discusses the subject of habitat preference, geographic origin and distribution, ecological relations (burrows, chimneys, etc.), and shows that there are three types of habitat preferences: (1) Those frequenting the larger rivers; (2) those frequenting small streams; and, (3) those frequenting springs and swamps. The river species are *Cambarus limosus*, *propinquus* and *obscurus*. These species are not completely restricted to these conditions, but occur also in any large permanent body of water, a habitat rarely found in Pennsylvania. The small or mountain-stream species is *C. bartoni*.<sup>1</sup> This form

<sup>1</sup> In southern Michigan (Ann Arbor) this species is abundant in small brooks flowing through open meadows and thus illustrates the habitat variability of this species.

avoids large streams and tends to push up stream into the head-waters, and further shows a preference for cool waters. The spring and swamp or burrowing species are—*C. carolinus*, *monongalensis* and *diogenes*. These species, says Ortmann, are always found at a certain distance from the open water, although often in close proximity to streams, ditches and ponds; but not in them under normal conditions. Ground water seems to be a dominant factor in their environment. It may be suggested that the amount of oxygen and carbon dioxide in ground waters, as well as a low temperature, may be intimately related to the habitat preference of these species. (This would be an interesting problem for an experimental study.) The haunts of the various species are discussed, including the shape of the burrows and the construction of the chimneys. The deeper parts furnish winter, and the upper parts summer quarters. The chimney is simply the by-product of the burrow.

The geographic relations of each species are summarized and discussed in detail; not only with regard to origin in the state, but also their general geographic origin and migration routes. The determination of these routes has required a knowledge of the history of the streams or drainage modifications in Pennsylvania and the adjacent regions. The original distribution of *limosus* seems to have been influenced by canals and mine drainage. This species is of special interest because it belongs to an isolated group both morphologically and geographically. The allied species occur in Missouri, Illinois, Kentucky, etc., and its most apparent route is through pre-glacial streams in the now glaciated region; while a northward route, from the south along the Coastal Plain is dismissed as unlikely. Ortmann concludes that *limosus* is a Tertiary type which was driven south along the Atlantic Coastal Plain, and within the interior, into Indiana and Kentucky, while the species was exterminated from the intervening region. It might be suggested that the preservation of this group of species at the extreme northern part of the Coastal Plain on the Atlantic coast, and the upper part of the Mississippi Embayment



may indicate a former southern, rather than northern connection. The southern route would give a continuous lowland habitat, and although it might mean competition with related species now, it does not follow that formerly there was such competition. Geologically speaking, both the glaciated region and the Coastal Plain are recently populated areas.

The geographic relations of *C. propinquus* and var. *sanborni*, and *obscurus* are discussed as a unit on account of their close affinities. Their history is quite involved on account of the complex histories of the streams they occupy. These closely related forms are representative of different parts of the Ohio system: *obscurus* of the upper Ohio, *propinquus* *sanborni* of the middle, and *propinquus* of the lower Ohio drainage. These respective sections of the Ohio are considered by physiographers as formerly parts of independent drainage systems which later became fused to form the Ohio, and consequently there was an opportunity for differentiation while the streams were separated. Even now while occupying the same system they show only a limited tendency to fuse. Their glacial preserve was apparently in the more south-central parts of the Ohio system, not far from the ice margin.

The species *bartoni* has the most extensive range in Pennsylvania, which is in decided harmony with its preference for small, rapid, and cool streams—such as abound in the Appalachians. This is apparently a preglacial species which has extended its range in post-glacial times to the northeast along the Appalachians, across various drainage lines—rather than along them. This is probably due to its tendency to frequent head waters, where divides themselves frequently migrate, and on account of the tendency and ability of this species to wander overland and thus 'migrate' the divides and around obstacles in streams, such as cascades or low falls. Such facts as these clearly emphasize the need of a detailed knowledge of the ecological relations of such animals before due weight can be given to the biological evidence suggesting physiographic changes.

This species becomes dwarfed in eastern Pennsylvania, a fact of much interest. There are so many conditions which may cause dwarfing that it would be of considerable interest to know what conditions have been operative here. The variety *bartoni robustus* is larger than the typical form and in Pennsylvania is confined to the extreme northwestern part, where the two forms occur together or separately. The general relations might be expressed thus: the largest form (var. *robustus*) occurs in the extreme northwestern part of the state, associated with or separately from a smaller form (*bartoni*) which alone occupies the intermediate eastern area of the state, and a still smaller or dwarfed form occupies the eastern part of the state. Thus there is a more or less progressive dwarfing to the eastward, if these forms prove to be intimately related.

*C. carolinensis* is restricted to the southwestern part of Pennsylvania, is a species of southern Appalachian range, and appears to prefer the higher altitude and clay soil of the Old Tertiary baselevel.

*C. monongalensis* also occurs only in the southwestern part of the state. This is a subterranean or burrowing species, even avoiding small streams, and thus it is not remarkable that the Ohio and Allegheny rivers have proved a barrier to its northward extension. The Youghiogheny and Monongahela rivers seem to have been traversed indirectly by the migration of a divide during the glacial period.

The range of *diogenes* is exceptional in that it occupies a narrow strip of the Coastal Plain along the Delaware river, is absent from all the central portion of the state, and occurs again in the southwestern part. Such discontinuity certainly suggests divergence as to origin. Ortmann is inclined to consider this species of Allegheny Plateau origin. A comparison made between the present range of *diogenes* and *limosus* shows that both exhibit a discontinuity of range between the eastern and western parts of the state, and both occupy the Coastal Plain. These facts suggest to Ortmann a retreat from the north before the glacial ice. Here again it may be



suggested that detailed investigation of the Coastal Plain may favor the hypothesis of a northward extension from the south. Such a possibility may be favored by a route across the Appalachians which seems to have been very generally overlooked as a highway—the Kanawha River Route—which reaches south into North Carolina. This is a stream with a remarkable history whose biological significance is worthy of detailed investigation, not only from the standpoint of aquatic invertebrates, but also of higher groups as well. (This is perhaps particularly true of birds.) In view of the antiquity of this route it seems very improbable that animals should ignore it.

In considering the crawfish characteristic of the natural physiographic regions of the state certain interesting correlations are evident. The Coastal Plain possesses two species, *diogenes* and *limosus*; the Piedmont Plateau and Great Allegheny Valley, which form a unit, are characterized by *limosus* and *bartoni*; the Allegheny Mountains by *bartoni*, with an invasion along the Susquehanna by *limosus* and an isolated colony of *obscurus*—both are due, Ortmann thinks, to the influence of man. A map of the state showing the location of past and present canals would greatly aid one in understanding the extent of this influence. The Allegheny Plateau, on account of its proximity and drainage into the great interior glacial preserve, has the most varied crawfish fauna—six species: *propinquus*, *obscurus*, *carolinensis*, *monongalensis*, *diogenes* and *bartoni*. It is thus seen that most of the species have invaded the state from the westward and that there is a marked attenuation of the fauna to the eastward.

The life histories of the species are considered in detail, the seasonal life history may be outlined as follows; a fall mating, a spring spawning season, an early summer season when the males are in the first form. The species which follow this cycle include, provisionally, *obscurus*, *propinquus* and var. *sanborni*, *limosus* and *diogenes*. These forms comprise what Ortmann calls the 'warm water' type and have a restricted breeding season. A second class includes those species which breed and spawn practically the year around, the

'cool water' type, and includes *bartoni*, *monongalensis* and probably *carolinensis*.

In *obscurus*, eggs are laid in April, hatch in May or June, and by September or October the crawfish are 40 to 55 mm. long, and the males are in the first form, the females are mature and copulation takes place from September to November. The winter is passed without change, and in April the females spawn and in June moult, the males having moulted in May. The fall moult ranges from August to October, and a second breeding season follows, and in the following year a third, after which they survive the winter, the males dying perhaps in early spring, and the females in June. They may thus reach the age of three years.

On account of the almost continuous breeding season of the 'cool water' species it is difficult to recognize broods and thus determine the detailed life history.

At present the known economic relations of crawfish are rather limited. They are used as food to a limited degree, especially about cities, but are very generally used as bait, hence the confusion which fisherman are likely to introduce into the problem of the original range of species. As scavengers they may be very beneficial, and occasionally dams may be injured, but these subjects have received but little attention by the public or naturalists. Their enemies include a wide range of animals, especially among the vertebrates.

The final section of Ortmann's paper is devoted to a discussion of the relation which this study of Pennsylvania species bears to the problems of evolution; mutation and isolation in particular. Ortmann criticizes de Vries's statement that species are formed only by mutation. He then considers in detail the evidence for marked variation or mutation among the Pennsylvania species and concludes that the differences between them are very constant and the variations slight, adding that "Anything that looks like a 'mutation' in de Vries's sense is entirely unknown." Even the differences between well-defined species "are so slight that they can not be regarded as representing 'mutations,' that is to say, sudden leaps in a progressive



direction." Rather than mutation Ortmann looks to isolation as the important factor and summarizes his position as follows:

1. The normal case is when two closely allied species, possessing identical or nearly identical ecological habits occupy separated areas, which lie close together but do not overlap. \* \* \*

2. Whenever allied species are found in one and the same locality (overlapping), isolation becomes apparent in the following forms:

- (a) The two species have different centers of origin, that is to say, they were separated formerly, but occupied the same territory subsequently. \* \* \*

- (b) If the centers of origin are more or less identical (absolute identity is hardly possible), the two species always differ ecologically, and although living at the same localities, prefer different surroundings. \* \* \*

So much for the general results of Ortmann's investigations, but such an outline does not throw into the foreground some of the features of the work which deserve special mention. This paper is devoted to the fauna of a limited area, and contains several of the elements which go to make up an ideal treatment of a local fauna. Not only is there an abundance of detailed facts, supplemented by a critical review of former records, but a very serious attempt is made to understand the meaning or significance of the details presented. Thus Ortmann's efforts to *interpret* and *correlate* the mass of facts is particularly commendable and is a marked departure from the usual annotated lists. He sees clearly that many of the facts to be explained involve a knowledge of the conditions which formerly existed and consequently he turns to a study of the determining conditions. The present work is also an important lesson on the value of a knowledge of the ecological relations of animals in the study of their relationships and geographical distribution, and clearly illustrates the difference between the older 'orthodox' zoogeography and the newer ecological phase.

Some suggestions may be mentioned which offer opportunities for improvement or expansion in future work. While the details of the environment of the burrowing species are described fully, the same information is needed

for the brook and river habitats; and such descriptions can be supplemented to advantage by photographs of representative situations. We need detailed maps showing the occurrence of crawfishes, in a manner similar to the mapping of plant societies by ecological botanists. The composite system of mapping used is not very satisfactory and would have been greatly improved had fewer species been placed on the map, and still more, if topographic maps had been used as a base. County and stream names are of great advantage on maps devoted to detailed faunal studies, and especially when the details of distribution are not expressed on the maps.

Ortmann does not seem to recognize, in more than a general way, the need of formulating the conditions which compose the most favorable (optimum) habitat, so that throughout the geographic range of a species the modifications of the habitat may be followed as definitely as structural and functional modifications (habits, etc.), so that eventually perhaps such relations may be correlated. In this connection it should be mentioned that the ecological observations are very largely from western Pennsylvania and should be extended over a larger area. But to do this the necessary time for *field study* as well as for *collecting* must be available. The importance of the laws of habit, habitat and environmental change is so great that it is very desirable that field students and institutional authorities see the real importance of these studies.

While reading this memoir a need has been felt for definite criteria by the aid of which some estimate could be made of the degree of primitiveness or specialization of forms. Further, if the general bearing of mutation is to be tested or used to the best advantage in the interpretation of the problems of habits, habitats and geographical distribution (of both plants and animals), it seems necessary to formulate criteria by means of which mutations can be recognized in nature (at least with some degree of probability), without recourse to pedigree cultures. Here, as when attempting to determine centers of origin, great care is necessary to avoid arguing in a circle.



Much confusion, both to the students of distribution and of pedigree cultures, has resulted from this lack of formulated criteria. Some may question the possibility of such criteria.

After such detailed studies, in which special emphasis has been placed upon geographic origin, one naturally expects certain criteria, perhaps more or less peculiar to the ecological relations of crayfishes, to be formulated, but such are not stated.

Two important papers should perhaps be mentioned in this connection, as they are not listed in the bibliography: Harris, 'An Ecological Catalogue of the Crayfishes belonging to Genus *Cambarus*' (*Kans. Univ. Sci. Bull.*, Vol. II., pp. 51-187, 1903), and Steele, 'The Crayfish of Missouri' (*Univ. of Cincinnati Bull.*, No. X., pp. 1-54, 1902).

A certain amount of statistical data could have been used to advantage. As this method of measuring variation, used with judgment and moderation upon critical phases and at critical localities, will aid such investigations. For example, if representative lots of *bartoni* from the northwestern, central and eastern parts of the state had been measured, the rate of dwarfing could have been determined. A similar comparison between the western and eastern variation of *diogenes* would be of value. Such variations as these are very common and signify to some that very frequently the species is too large a unit for the study of geographic distribution, that local variations or races are of great importance and that in further investigation the forms peculiar to definite habitats should receive recognition and detailed investigation.

In conclusion it should be said that such excellent work, perhaps the most important general zoological work yet published by the Carnegie Museum, should be continued, as the subject has reached such a degree of development that to stop now would be unfortunate, to say the least. The region to the south and west should now be considered, not only because of its proximity to Pittsburgh, but primarily because it is apparently in that direction that a most wonderful evolution of crayfishes has taken place, or is taking place. Then with the modification of original condi-

tions through the 'improvement' of streams for navigation, water power and supplies, the construction of canals, contamination by industrial refuse and sewage, we have additional urgent reasons for an early continuation of such investigations so that 'vanishing data' will be preserved for future generations. The Carnegie Museum is not a provincial institution, and does not necessarily limit its activities to the state of Pennsylvania, and it is hoped that this work will be continued, as the present study has clearly shown that the most important part of the problem still awaits detailed investigation.

CHAS. C. ADAMS

UNIVERSITY OF CINCINNATI,  
CINCINNATI, OHIO

#### SCIENTIFIC JOURNALS AND ARTICLES

*The Journal of Comparative Neurology and Psychology* for March contains a report of the convocation week meetings held in New York city during the winter, including abstracts of most of the papers read before the various societies in the fields of neurology and animal behavior. The leading article is a memoir on 'Light Reactions of *Volvox*,' by S. O. Mast. The light reactions were studied under rigidly controlled conditions in the 'light grader' devised by the author. Among other results, it was found that the direction of motion in *Volvox* is regulated by the relative light intensity on opposite sides of the colony regardless of the ray direction. Orientation is not the result of 'trial and error' reactions, as in *Stentor*, *Euglena* and other forms. *Volvox* colonies make no errors in this process. There is no evidence of motor reaction in a *Volvox* colony, taken as a whole. Orientation is, however, brought about by motor reactions in the individuals which constitute the colony. Weber's law holds approximately for the light reactions of *Volvox*.

*The American Naturalist* for April has for its leading article a discussion of 'The Geographic Distribution of Closely Related Species,' by Robert G. Leavitt. The question is considered from a botanical standpoint and the author's conservatively stated conclusions



are in favor of the mutation theory of the origin of plant species. 'The Coincident Distribution of Related Species of Pelagic Organisms as illustrated by the Chætogonatha' is by Charles A. Kofoid, who shows that there is a tendency for two species of a genus of this group to occur in one locality and not elsewhere, and considers that this casts some doubt on the universality of operation of isolation in the evolution of species. E. A. Andrews describes at some length 'The Attached Young of the Crayfish, *Cambarus clarkii* and *Cambarus diogenes*' and considers their bearing on the question of the evolution of the species.

*The American Museum Journal* for May is mainly devoted to an article by Clark Wissler on 'The Douglas African Collection' recently acquired by the museum through the generosity of some of its friends.

*The Bulletin of the Charleston Museum* for April contains an account, by Ezra Brainerd, of 'A Visit to the Gravé of Thomas Walter,' one of the earliest of American botanists and the author of *Flora Carolina*. A pleasant result of this visit has been the taking of steps for the preservation and protection of the grave.

*The Peabody Museum of Natural History*, Yale University, has just issued Guide No. 1 on 'The Evolution of the Horse Family,' by Richard S. Lull, and based on the valuable material mainly brought together by Professor Marsh, and recently admirably arranged and labeled by Dr. Lull.

#### SOCIETIES AND ACADEMIES

##### THE AMERICAN CHEMICAL SOCIETY. NEW YORK SECTION

THE seventh regular meeting of the session of 1906-'07 was held at the Chemists' Club, 108 West 55th Street, on May 10.

The following papers were presented:

*The Causes of the Corrosion of Iron and Steel:* W. H. WALKER.

With the ever-increasing use of iron and steel, the conditions which limit the life of structures made from these materials, assume

great importance. To few subjects have been devoted so much elaborate investigation with such conflicting results. Of the many papers which have been published and theories advanced as to the cause of corrosion, the three following are of special importance: Calvert, after a series of experiments came to the conclusion that ordinary corrosion or rusting of iron could take place only when all of the three reagents, carbon dioxide, water and oxygen were present. This opinion was universally accepted until in 1903 Whitney showed that corrosion was a purely electrochemical phenomenon and would take place in water in the absence of both oxygen and carbon dioxide, although for the formation of the so-called rust, oxygen was necessary. A year or so later Dunstan and his co-workers published the results of their work from which they concluded that Whitney was at fault and that iron was not corroded by water in the absence of oxygen and carbon dioxide. They believed the action of oxygen on iron to be a direct one, with the intermediate formation of hydrogen peroxide, and that in ordinary corrosion electrochemistry does not play a part. Following this paper came one from Moody, of Kensington, England, who took issue with both Whitney and Dunstan and describes experiments which he thinks conclusively prove that no corrosion of any kind takes place in the total absence of carbon dioxide.

Work recently carried on at the Institute of Technology substantiates Whitney's claim in so far that there is a slight corrosion of iron in pure water although if oxygen and carbon dioxide be most carefully eliminated, the presence of dissolved iron can be detected only with the greatest care, and possibly if these two constituents were absolutely removed, no iron would be dissolved. There is a tendency, however, for iron to pass into solution and for hydrogen to precipitate out in a way analogous to the action of iron in a copper sulphate solution. Unless oxygen or some other substance be present to unite with the hydrogen when set free upon the surface of the iron, the action, if it starts at all, very soon ceases. To remove this hydrogen and thus accelerate the action is the function of oxygen in corrosion.



Experiments show that the rapidity of corrosion is directly proportional to the partial pressure of the oxygen in the atmosphere above the water containing the iron and therefore in the water. A reagent which indicates very clearly those portions of the iron at which iron ions are passing into solution, on the one hand, and where hydrogen is passing out of solution with the formation of hydroxyl, on the other hand, was found in ordinary tap-water containing a little phenolphthalein and potassium ferricyanide. The red and blue zones are quickly apparent when any piece of iron is immersed in this solution, and can be rendered more or less permanent if the solution be thickened with gelatin or agar-agar.

The potential difference which has often been observed between iron and iron oxide, is shown to be occasioned by the unequal condensation of oxygen upon the two surfaces. For example, magnetic oxide of iron which, under ordinary circumstances in connection with iron, shows a large difference of potential in water or weak electrolyte, indicates no difference when the system is entirely free from oxygen.

Differences of potential which can easily be observed upon different portions of an iron plate, may be also explained by the varying capacity of different portions of the iron for occluding or segregating oxygen.

The electrochemical theory is substantiated by showing that any reagent which increases the concentration of the hydrogen ions will increase the rate of corrosion, while reagents which decrease this, inhibit corrosion. The fact that bichromate and chromic acid inhibit rusting, may be explained by the formation of an enclosing film of oxygen evenly distributed over the surface in a way analogous to that which may be formed by immersion in nitric acid.

*The Analysis of Chlorides and Sulphocyanate Mixtures:* M. A. ROSANOFF and ARTHUR E. HILL.

To analyze chloride and sulphocyanate mixtures is to-day a difficult matter. The known gravimetric methods are laborious. The known volumetric methods are but little

more rapid and far from precise. The authors have devised a new volumetric method, which is easy of execution and yields results of the highest precision. Its basal facts are as follows: (1) At the temperature of boiling water soluble sulphocyanates are readily oxidized by small quantities of nitric acid; (2) most of the hydrocyanic acid produced can be expelled in a short time by boiling; (3) no hydrochloric acid is lost, owing to complete electrolytic dissociation; (4) silver cyanide is somewhat soluble, silver chloride is insoluble, in moderately dilute nitric acid.

The method can be used to determine chlorides in the presence of both sulphocyanates and cyanides, and the authors are endeavoring to extend it to the determination of bromides. Details of the *modus operandi* will shortly appear in the *Journal of the American Chemical Society*.

*Gasoline-Soap 'Emulsions' and their Relation to Sewer Explosions:* A. A. BRENNEMAN.

The 'emulsion' produced by shaking up gasoline or benzine with soap solutions gathers slowly upon the surface of the aqueous liquid forming a thick, creamy paste which is very permanent in closed vessels but disintegrates rapidly on exposure to air by volatilization of the hydrocarbon. The ordinary operation of washing the hands with soap after benzine or gasoline has been used to remove grease, carries off the light liquid as an emulsion. This same emulsion can be made in quantity by shaking up gasoline with a weak soap solution in a stoppered glass cylinder. It then rises in an hour or less to form a thick, white, creamy layer which can be drawn off and kept separately. In this condition it is very permanent, requiring many days to effect an appreciable further separation. Under the microscope it shows a mass of air bubbles studded or coated with minute globules of gasoline. The air within the bubbles is saturated with vapor of gasoline, the soap solution is indifferent to it, and vapor pressure is at an equilibrium throughout the system. The permanence of the mass in a closed vessel is therefore to be expected. In the open air it disintegrates rapidly, giving off gasoline



vapor. The entrance of this material into drains and sewers where gasoline and soap are used for washing, as in garages and factories, is sufficient to account for the liberation of much combustible vapor and hence, perhaps, for explosions. Such material separates slowly and is difficult to trap.

Professor Breneman also read two 'laboratory notes,' one relating to the magnetic quality of magnetic (iron) oxide in the hydrated state, and one upon the use of ether in the ferric sulphocyanate test.

C. M. JOYCE,  
Secretary

#### THE TORREY BOTANICAL CLUB

The club was called to order on February 27, 1907, at 3:30 P.M., at the Museum Building of the New York Botanical Garden, with Dr. William A. Murrill in the chair. Twenty-one persons were present.

The following scientific program was presented:

*Tubular Glands in the Corn Embryo:* C. STUART GAGER.

The literature dealing with the transformation of starch to sugar in the corn grain during germination was first briefly reviewed, and its bearing on the structural anomaly subsequently described was pointed out. This anomaly consisted of invaginations of the glandular epithelium of the scutellum into the tissue of the latter, in such a way as to form true glands of the tubular and subracemose type.

The significance of these glands, as in harmony with the theory that the scutellar epithelium is principally an organ of secretion, was also indicated. The paper was illustrated by microscopic preparations and photomicrographs, and will be published in full in the *Bulletin* of the club for March, 1907.

A brief discussion followed.

*Explorations in Southern Florida:* JOHN K. SMALL.

The exploration was confined to the larger group of islands lying between Miami and Camps Longview and Jackson, and to a wholly unexplored section of the everglades

lying between the present terminus of the Florida East Coast Railway and Key Largo, including a portion of Cross Key. This latter island, together with a parallel and almost similar formation, constitutes the only natural and approximately complete land-connection between the Florida Keys and the mainland of the peninsula. The chain of everglade keys is a miniature of the Florida Keys, both in its crescent shape and its flora, and also of the West Indies in the character of its vegetation. It is surrounded by the everglades, except where the upper islands touch Biscayne Bay at points from Miami to Cutler. Before these islands were elevated to their present altitude, they were probably surrounded by a shallow sea, just as the Florida Keys are at the present time. This being the case, the tropical American flora now inhabiting them may easily be accounted for. After sufficient elevation had taken place, the surrounding sea was transformed into the vast spring now known as the everglades. Conditions becoming favorable, the plants of the flora of northern peninsular Florida advanced southward and naturally took complete possession of the area that was formerly the sea, thus surrounding and isolating the wholly different flora of the islands. In fact, the two floras are so sharply delimited that one can often stand with one foot on plants characteristic of the high northern regions and the other on plants restricted to the tropics. It is not an uncommon experience to see colonies of plants common in Canada, such as the arrowweed (*Peltandra*), the lizard's tail (*Saururus*) and the ground-nut (*Apios*), growing side by side with tropical palms, cycads, orchids and bromeliads.

The total area of these islands is perhaps about one hundred and fifty square miles. Those that have been explored have yielded between five and six hundred species of native flowering plants, surely a very large number considering the fact that the solid rock is exposed everywhere and that soil in the ordinary sense of the word does not occur there. The close relationship of this flora to that of the West Indies is now established by the fact that considerably more than one half of the



species found on the islands south of Miami are also native in Cuba and the Bahamas.

Since the publication of Dr. Small's last report on exploration in southern Florida, and a subsequently printed paper on the species added to the flora of that state, he has secured over fifty more species not before known to grow on the North American mainland. Eight or ten of these are complete novelties, inasmuch as they are not yet described. Noteworthy among the recent collections, which make an aggregate of 3,200 specimens, are seven species not previously included in the tree flora of the United States.

After an interesting discussion of Dr. Small's paper the club adjourned at five o'clock.

C. STUART GAGER,  
Secretary

#### THE BIOLOGICAL SOCIETY OF WASHINGTON

THE 430th meeting was held April 20, 1907, with President Stejneger presiding.

The first paper, by Mr. George B. Morse, was entitled 'Preliminary Observations on the Quail Disease in the United States.'

The speaker quoted from a booklet entitled 'Quail Culture from A to Z,' published in 1905; "There is no contagious disease among quail that has yet made its appearance. \* \* \* They have lice, but not disease." The facts recorded in these observations are a complete refutation of that statement. In April, 1906, there were received from a Washington dealer three dead bobwhites, the last of a large number that had been steadily falling victims to a highly contagious and rapidly fatal disease. In May, 1906, and January, 1907, letters were received from Boston, Mass., and Worcester, Mass., referring to what was undoubtedly the same disease. From February 11, 1907, to March 21, 1907, two dealers in Washington lost upwards of 250 bobwhites, and quite a number each of several other species of quail. Post-mortem examination revealed the same lesions in all. The sources from which these birds were received demonstrated as known centers of infection Alexander City and Dadeville, Tallapoosa County, and Birmingham, Jefferson County, Ala.; Wichita, Kans.; and

Marlow, Chickasaw Nation, Ind. T. In addition to the above, other localities such as Washington, D. C., Boston and Worcester, Mass., Elizabeth, Pa., and Yarmouth, Nova Scotia, have become more or less infected by means of shipments of diseased birds received. The disease has been thus far demonstrated in the following species: bobwhite (*Colinus virginianus*), California quail (*Lophortyx californicus vallicola*), Gambel quail (*Lophortyx gambeli*), mountain quail (*Oreortyx pictus*), scaled quail, 'cotton-top' or blue quail (*Callipepla squamata*) and the sharp-tailed grouse (*Pediocætes phasianellus campestris*).

Period of incubation appears to be about ten days. Symptoms are: dullness, fluffed feathers, neglect of food. In acute cases (the most common) death occurs within two or three days. In chronic cases diarrhœa occurs and emaciation is extreme. At post-mortem examination the characteristic lesions are pulmonary congestion, superficial necroses of the liver and intestinal ulceration. Bacteriologic investigation of the cases studied in 1906 resulted in the isolation of a bacillus apparently identical with Klein's bacillus of grouse disease. The cases studied in 1907 yielded with striking unanimity a variant of *Bacillus coli* with which the author has produced death in mice, guinea-pigs and bobwhites with the characteristic lesions. The disease was therefore spoken of as an infectious disease of the grouse family produced by a member of the *B. coli* group, described in circular No. 109, of the Bureau of Animal Industry. No curative treatment was offered but procedures for prevention were outlined, methods applicable to the prevention of disease of intestinal origin among all wild birds brought under habits of life more restricted than those normally enjoyed.

Dr. T. S. Palmer referred to the importance of the establishment of this disease among American quail, as the grouse disease is established in Europe. When first heard of last autumn it was supposed the grouse disease had been imported. During the past ten years there has been a marked decrease in abundance of quail, particularly following severe winters and there is a large demand for birds to re-



stock, reaching from 100,000 to 200,000 annually. Shipments from the supply centers, as Texas, Indian Territory and Alabama, are liable to be centers of quail disease infection, and in this case shipments may be discontinued by law.

The second paper was by Dr. F. V. Coville on 'Photographic Reproduction of Rare Botanical Books.' He referred to the desirability of having reproductions of rare and valuable works to which frequent reference is made, in order to preserve the originals. These were made by photographing each page and binding the resulting prints into a book. Specimens of such reproductions were exhibited. These had been made for actual use in botanical work, some of them so closely simulating the original as to scarcely show they were photographs. Pages yellowed by age, however, show black or dark in the reproduction.

The third paper, by Mr. R. E. C. Stearns on 'The Composition and Decomposition of Fresh Water Mussel Shells, with Notes and Queries,' was read by Dr. Palmer in the absence of the author. It will be published in full in the *Proceedings* of the society.

M. C. MARSH,  
*Recording Secretary*

#### THE PHILOSOPHICAL SOCIETY OF WASHINGTON

THE 634th meeting was held on April 27, 1907, President Hayford in the chair. The society heard an address by Dr. H. W. Wiley, upon the subject of 'How Much Do We Eat?' After briefly mentioning, without discussion, the three principal schools of philosophy relative to man's food consumption, the speaker proposed three points of discussion having a direct bearing upon the subject of how much do we eat, *viz*: the proper proportions of food necessary for growth; for equilibrium of weight, and for old age; the second only of these three was discussed at length. The speaker reviewed briefly the experiments carried out under his direction, where records have been kept for nearly five years of the quantities of food eaten by healthy young men. Some fifty or sixty young men have

been under observation during this period, and all the food which they have eaten has been carefully weighed.

In the fore period, preliminary to the observations, the ration which the young men would normally choose was changed one way or the other in order to secure the equilibrium desired. The proportion of protein to the other elements of the ration was selected by the normal taste of the subject, save in some instances where there seemed to be a tendency to eat too much meat, this was slightly checked. A sample of all the results shows that the dry food eaten by a man each day is almost one per cent. of the weight of the body. In other words, a young man weighing 150 pounds will eat in twenty-four hours 1.5 pounds of dry food. The weight of moist food, including water, is almost exactly 4.25 per cent. of the weight of the body. The total amount, therefore, of food and drink in the state in which the food is consumed for a young man of 150 pounds in twenty-four hours is about 6.4 pounds. In other words, the amount of water taken in his food and drink during the day is nearly 5 pounds.

Important questions of social and scientific character arise in connection with the magnitude of the diet. Interesting observations have lately been made looking to the diminution of the quantity of food eaten per day. Mr. H. Fletcher has made interesting observations on this subject, and has sought to show that the quantity of food ordinarily eaten is too great. He calls attention to the fact that slow and patient mastication may suffice to make a less quantity of food satisfy hunger, and furnish the necessary heat and energy for the ordinary human activities. Mr. Fletcher himself submitted to experimental investigations in the calorimeter at Middletown, Conn. The data furnished by the calorimeter indicated that more heat was evolved than could possibly have been furnished by the quantity of food claimed to be eaten.

Of course, it is not possible that a man may live without damage on less food than would furnish the heat and energy for the ordinary activities of life. There must necessarily in this case be a waste and the waste



can only be made from the tissues themselves.

The recent investigations of Professor Chittenden must be taken into consideration, where it was demonstrated that strength and body equilibrium could be secured by cutting down very materially the nitrogenous part of the ration. Some of these experiments were continued over a long period of time, and showed that strength even increased with the notable diminution of the nitrogenous elements consumed. This is all interesting, but probably not convincing. If we, for the sake of argument, assume that the theory of evolution is a correct one, then we must admit that man to a certain degree is a creature of his environment. Experience shows that when the human animal is allowed to choose his ration with reasonable facility to get what he wants he eats a certain weight of food in which there is a certain proportion of nitrogen, which it may be said for a man of 150 pounds is not far from 18 grams per day. What would be the effect upon the human animal of cutting this nitrogen out by one third or one half in the course of a few generations or of a few thousand or hundreds of thousands of years? It would, perhaps, change in a very marked degree the human animal. That change might be possibly for the better, but certainly it would not represent the animal himself as he is to-day.

I have just read in the newspapers, which are not always the most reliable purveyors of scientific information, that the recruiting officers in the German Empire have found very few young men in a certain locality suitable for military service, and the inference is that the high price of meat has probably excluded it from the ordinary diet of the peasant, so that the children of the peasants are not receiving the amount of meat food, and presumably of nitrogenous material, which they formerly were able to get. This report, of course, is not worthy of being considered from a scientific point of view, but it shows at least an indication of the trend of thought in this matter.

The best nourished nations, as a rule, are foremost in literature, science and arts, and, according to numbers, in physical power.

Those who treat of diet from an economic, as well as scientific point of view, should be very conservative in advocating any change in rations which would lead to a minimum diet naturally chosen or to a reduction of the proportion of nitrogen to the other constituents therein.

R. L. FARIS,  
Secretary

#### DISCUSSION AND CORRESPONDENCE

##### A PROTEST ON BEHALF OF THE SYSTEMATIC ZOOLOGIST AND THE BIBLIOGRAPHER

A PAPER recently come to hand on the Nearctic Hemerobiidæ, *Transactions of the American Entomological Society*, XXXII., pp. 21-52, furnishes an opportunity for a criticism that is not intended for the author in particular, but as a protest against a particular kind of carelessness that we meet with too frequently in present zoological literature. On page 40 of that paper is described what appears to be a new genus, and is so indicated by the abbreviation 'n. gen.' placed after the name. No other reference to the use of the name is indicated. Any bibliographer or future worker would be very justifiably led into the error of dating this genus, and of the several others in the paper which are all treated in the same way, from December, 1905, the date of the paper. But on turning to page 46, we are told in a brief appended note that Dr. Needham has in July, 1905, described this genus under another name. It is then explained that the author published the name of this genus, as well as of the others published in the paper under discussion, in connection with the name of a described species, as early as November, 1904, and that therefore Dr. Needham's name is a synonym. I find no fault with this conclusion, but why I ask, and I demand it in the name of the systematist and of the bibliographer, does he not indicate the date from which the genus originates in the early part of his paper? Why does he indicate as a new genus that which from the standpoint of nomenclature he has described a year earlier?

Take another instance. Dr. Ashmead in



his classification of the Ichneumonoidea published a few years ago has described genus after genus designating manuscript species as types and connecting no known species with them. Dr. Ashmead doubtless intended to describe these species. But he has never done so, and we learn with profound regret that his health is such that he never will be able to do so. Now what is going to be the status of these genera? There will be those who, interpreting strictly the laws of priority, will ignore them absolutely, on the ground that they are *nomena nuda*. There will be others who will attempt to assign them to this place or that, but no one will ever know what their author intended, unless some one, with this purpose in view, laboriously works over the collections on which Dr. Ashmead has based his names. Even then no agreement will be reached among future students as to what is to be done with these genera, which number no less than forty-eight, and like those of Forster they will remain for years a source of confusion, error and instability in our nomenclature.

Instances might be multiplied, but these will suffice, for I do not intend them as personal criticisms, rather merely as remonstrances against a too prevalent carelessness on a very important subject. In a day when the difficulties of the application of the laws of nomenclature, and the increasing confusion in zoological nomenclature are being continually brought home to us on every hand, are such practises on the part of those who are certainly by no means amateurs in systematic zoology to be condoned?

J. CHESTER BRADLEY

UNIVERSITY OF CALIFORNIA,

April 24, 1907

#### SCIENCE AND POETRY—A PROTEST

THE advisability of correlating literature and science in the schools was at one time a much-debated educational question. The writer has heard seriously advocated before a State Science Teachers' Association the advantage of always having the zoology class read 'The Chambered Nautilus' when studying the Mollusca, though assent was withheld

by the same speaker from the proposition to have the members of every English literature class dissect a nautilus when studying Holmes's poem. That there is nothing poetical in the bare facts of nature, and that nothing is really interesting unless invested with poetry or fancy, are two ideas that can never, it seems, appear erroneous, except to one who has studied nature at first hand.

Sugar-coating the supposed pills of scientific fact in nature-study literature and teaching has been baneful enough, but when articles in reputable magazines, intended for mature minds, poeticize science to the verge of misrepresentation, it is difficult to know whether to blame the author the more, or regretfully to decide that, after all, the general public is still unable to appreciate natural facts as nature presents them.

A series of three articles in *Harper's Monthly Magazine* for December, 1906, and February and March, 1907, entitled 'The Intelligence of the Flowers,' by Maurice Maeterlinck, have been the inspiration of the protest.

To say that no flower is 'wholly devoid of wisdom'; that, in order to deprive a flower of reason and will, 'we must needs resort to very obscure hypotheses'; that it is in the vegetable world that 'impatience, the revolt against destiny, are the most vehement and stubborn'; and that the pollination of the eel-grass is 'a tragic episode,' may be most excellent poetry, and enhance the literary value of an article; may, indeed, for aught we know, be the necessary conclusions of a poet, but to read such statements in cold print congeals the blood of any botanist.

Still we might shiver in charity if interpretations only, and not facts, were open to question. We are told, for example, that the tip of the young stem of a seedling laurel tree, because the seed germinated on a perpendicular rock-wall, 'instead of rising towards the sky, bent down over the gulf,' notwithstanding its geotropism.

We learn that dodder 'voluntarily abandons its roots,' and that it will avoid other species and, 'go some distance, if necessary, in search of the stem of hemp, hop, lucerne or flax.'

In the second article we learn, for the first



time, that the *flowers* of *Drosera* and *Nepenthes* are carnivorous, and that the problem of cross-fertilization is 'normally insoluble.' Here, also, obsolete terminology is perpetuated in the expression 'fertilization of the stigma,' and obsolete interpretation in referring to the stigma as the 'female organ,' and to the stamens as the 'male organs' of the flower.

The fact, stated in the first article, that the Virginia creeper or the convolvulus will begin to twine about the handle of a rake, temporarily laid against a wall, does not seem, in the author's mind at least, at variance with the clear 'perspicacity,' 'intelligence' and 'prudence' with which plants in general are attributed elsewhere in the articles. One wonders, though, why the convolvulus did not 'set its thought to working,' as did the *Silene Italica*, mentioned a few lines farther on. But doubtless we have failed to enter into the spirit of the author, for later he implies intelligence to the mountains, the seas and the stars.

'The flowers,' we are told, 'came upon our earth before the insects.' This 'geologically incontestable fact' is, alone, 'enough to establish evolution'!

But the discoveries of recent science sadly pale in comparison with the root-intelligence described in a foot-note to the first article, and credited to Brandis. Thus:

This root, in penetrating into the earth, had come upon an old boot sole: in order to cross this obstacle, which, apparently, it was the first of its kind to find upon its road, it subdivided itself into as many parts as there were holes left by the stitching needle; then, when the obstacle was overcome, it came together again and reunited all its divided radicles into a single and homogeneous tap-root.

Of course no one could state, *a priori*, that such a marvelous feat was impossible, but it is the kind of tale to which one more readily gives credence if substantiated by photographic evidence. Without such evidence the event, as narrated, is absolutely incredible to

<sup>1</sup> That insects appeared in Silurian times, and that there is no certain evidence of angiosperms earlier than the Cretaceous, are facts of paleontology too well known to be dwelt upon here.

any botanist. But even if such an act were common for roots, by what stretch of the imagination could one infer that a root could have preconceived and reasoned out the plan so deftly executed?

There is much in these articles of interest, and of scientific accuracy, and the apparent appreciation, in the last one, of the value of the experimental study of variation is very gratifying.

"All that we observe within ourselves," says Maeterlinck, "is rightly open to suspicion; and we are too greatly interested in peopling our world with magnificent illusions and hopes." Perhaps this explains the impossible botany of the articles, but it can not excuse it.

C. STUART GAGER

NEW YORK BOTANICAL GARDEN,

April 30, 1907

#### CONCERNING LEFT-HANDED ABORIGINES

A RECENT article in *SCIENCE* requested people in charge of Indians to find the proportion of left-handed aborigines to the right-handed ones. Acting upon that request, the writer has been investigating the subject among the Hoh and Quileute Indians, and, out of a population of 231, five left-handed people were found: How-withlup (male), Wallo-thlu (male), Hick-sh (male), Thle-ba-tolch (male), Hi-yic-to-utl (female).

ALBERT B. REAGAN

LA PUSH, WASH.

#### UPLIFT INCREASES RAINFALL, DENUDATION DIMINISHES IT

It has long been known to students of geography that in most parts of the world more and more rain and snow is observed to fall as one examines greater and greater heights on the slopes of hills and mountains up to very considerable elevations. Hellmann's new rainfall map of Germany shows this to be true even of the very flat hills on the plains of northern Prussia. At any point on this plain the hills are a little wetter and the valleys drier than the ground about. Dr. Kassner has suggested in the February *Petermann* that in regions of subdued mountain form there must,



therefore, have been greater rainfall in the past when erosion had not accomplished so much of its leveling effect, and remarks that a map of that old-time distribution of rainfall is capable of construction on the basis of the approximate land elevations of the land before denudation took place. In this sense the denudation of the land has been accompanied by diminution of precipitation. It should be remembered, however, that regional uplift has the opposite effect and has not infrequently been the occasion of increase of rainfall and denudation. The Black Hills of South Dakota, for instance, have more rainfall than the region about because of the domed uplift of the region above the plains. It is estimated that 3,000 feet have been removed from their summits by denudation since this uplift and this Kassner would suggest must have been accompanied by diminution of rainfall. But it is quite conceivable that the summits have never been more than 700 feet above the sea, for denudation has been lowering them at the same time that doming has thrust them up. In that case there has been no reduction in height or diminution of rainfall. When uplift ceases and denudation alone controls the elevation, rainfall must undergo the diminution spoken of, but the complete cycle of changes began with increase of rain as the doming first began. This supplied the abundant transporting agent with which erosion resisted further effective uplift and brought increase of precipitation to a halt much as the governor controls the throttle of an engine. From what we may call a mature stage of rainfall, reached likely enough in geographic maturity of the mountain forms, Kassner's diminution must come in.

MARK S. W. JEFFERSON

YPSILANTI, MICH.

#### SPECIAL ARTICLES

##### THE DEVELOPMENT OF UNFERTILIZED FROG EGGS INJECTED WITH BLOOD

DURING three successive springs (1905-7) the writer has experimented on unfertilized frog eggs by injecting them with blood or lymph

of either male or female frogs. In all some fifteen hundred eggs have been so operated upon. Shortly before the time for laying, the eggs were taken from the uterus with every precaution to prevent contamination by sperm. Those nearest the cloacal opening were always set aside as a control and in not a single instance did any of them develop. The other eggs were pricked with a very fine-pointed capillary tube which had previously been charged with lymph and corpuscles by dipping it into the lymph or the blood of another frog.

In eggs so treated numerous instances of cell proliferation and embryonic development have been observed, provided the eggs were fully matured and ready for fertilization. Many eggs after six or eight days showed upon sectioning that they had approximated the full blastular and in some cases the gastrular stages, although the condition came about apparently by some sort of internal nuclear arrangement, as no superficial cleavage furrows were observable and no demarcation into cells was visible from the exterior until the third or fourth day, when close inspection showed in some cases numerous small vesicular or cellular outlines.

In some instances definite organs were developed, though frequently distorted and misplaced. Cross-sections of one embryo, for example, showed such pronounced defects as two neural tubes anteriorly. Of the whole number of eggs operated upon only two developed into free-swimming tadpoles and these were apparently normal as far as superficial examination disclosed. They have not yet been sectioned. After sixteen days one died and the other was killed to insure proper fixation for histological study.

Apparently the white rather than the red corpuscles are the stimulating agents which bring about development, because injections of lymph, which contains only white corpuscles, produce the same effect as injections of blood. Whether or not the fluid part of the lymph or blood produced any effect could not be definitely determined from the material at hand. The whole effect seems, however, to be the result of the proliferation of the leuco-



cytes themselves, which, as they become more numerous, tend to migrate to the surface of the egg and finally form into one or more layers. Each nucleus apparently acquires a local area or zone of protoplasm which ultimately becomes marked off from adjacent areas as more or less of a definite cell. The pigment of the egg accumulates around the boundaries of the more superficial areas, which thus appear to be sharply delimited, as seen in sections under the microscope. Although the internal mass of yolk contains numerous nuclei, frequently undergoing amitotic division, the central mass of the eggs remains in a syncytial condition for considerable time.

I am inclined to believe that, in some cases at least, the female pronucleus of the egg takes no part in this cell proliferation, because I have been able to find in sections in several instances a comparatively large clear protoplasmic zone, variously placed in the egg, which, although invaded more or less on all sides by nuclei, itself remains undivided, and in it is visible what appears to be a degenerating nuclear-like structure, presumably the remainder of the female pronucleus.

Many eggs show no development, but this is not to be wondered at, since doubtless a number of them, although pricked, received no corpuscles from the orifice of the capillary tube. Other eggs, presumably not fully ready for fertilization, did not develop; although the corpuscles apparently proliferated extensively, they later ran together to form giant cells and frequently seemed to become phagocytic in nature. In still other cases, what seemed to be phagocytosis was visible on one side of the egg, while on the other side the nuclei appeared to be ranging up into a definite cellular layer. A detailed description of the experiments is in preparation.

MICHAEL F. GUYER

THE UNIVERSITY OF CINCINNATI,

May 13, 1907

TRANSMISSION INHERITANCE DISTINCT FROM  
EXPRESSION INHERITANCE<sup>1</sup>

CONJUGATIONS of sex-cells of higher plants and animals have two results, an intermediate

<sup>1</sup> Read before the Botanical Society of Washington, April 13, 1907.

and a final product. The intermediate product of conjugation is a new organism, the final product a new equipment of sex-cells. The new organism is built up by vegetative subdivisions of the conjugating pair of sex-cells. The conjugation is not completed until the new generation of sex-cells is to be formed. Fertilization is the beginning of the process of conjugation, which may not conclude for months or years after fertilization has taken place. The organism which is built up during conjugation may be called a conjugate organism, or *conjugate*. It belongs to the same generation as the sex-cells which initiate the conjugation. The next generation may be called *perjugate*, since it has passed through the conjugation of the preceding generation and represents its completed results.

When sex-cells of diverse parentage are associated in conjugation the organisms they build up (conjugates) may be like one parent, or like both parents, or intermediate between the two parents, or different from either parent. The same latitude of alternatives of expression is found in the *perjugate* generation. The crossing of two varieties of pink-eyed mice yields black-eyed conjugates. Two varieties of smooth-seeded cottons gave smooth-seeded conjugates, but woolly-seeded *perjugates*. Such instances prove that the expression-tendency of a gamete can be altered by association with another gamete of diverse parentage. Either the conjugate generation or the *perjugate* generation, or both, may show characters which neither of the parent gametes would have brought into expression if it had secured a partner of its own kind. There is no corresponding proof that transmission inheritance is altered by such associations. The reappearance of such characters as the black eyes and the woolly seeds, which have been abeyant through many generations, shows that failure of expression does not prove failure of transmission.

Transmission inheritance may be thought of as the dial of a compass which carries many character-directions, though the needle of expression points to only one. This expression-polarity is called dominance in conjugate organisms and potency in gametes. Nobody



denies the transmission of unexpressed characters through conjugate organisms, but in dealing with gametes the distinction between transmission and expression has continued to be overlooked; otherwise the Mendelian hypothesis of pure germ-cells could not have attained its wide popularity.

Mendelism and other forms of polarized expression inheritance yield us no intimation whatever regarding the nature and mechanism of transmission inheritance. If transmission could be conceived as a matter of localized character-unit particles we should be justified in thinking of all germ-cells as containing full sets, and not variously mangled fractions of the ancestral equipments. Alternative inheritance of divergent characters means reciprocal expression-polarities. It has yet to be shown that there is any such phenomenon as alternative transmission inheritance, brought about by the segregation of the parental character-units in different germ-cells. Incompatibility sufficient to cause germinal segregation should preserve the original association of the characters, but no such tendency has appeared in Mendelian crosses. When there are several divergent characters they are always expressed in many different combinations, as though to show that the scale of transmission remains complete, no matter how narrowly the needle of expression may sometimes be directed.

O. F. COOK

#### A NEW METHOD BY WHICH SPONGES MAY BE ARTIFICIALLY REARED<sup>1</sup>

I HAVE found in the course of an investigation carried on for the Bureau of Fisheries that silicious sponges when kept in confinement under proper conditions degenerate, giving rise to small masses of undifferentiated tissue which in their turn are able to grow and differentiate into perfect sponges. The investigation has been prosecuted during the past three summers at the Beaufort Laboratory. While the degeneration with the formation of the indifferent masses has been ob-

<sup>1</sup>Published with the permission of Hon. Geo. M. Bowers, U. S. Commissioner of Fisheries. served in several species, it is only in one

species, a *Stylotella*, that the process as a whole has been worked out.

This sponge, which is exceedingly abundant in Beaufort Harbor, is a fleshy monactinellid commonly reaching a thickness and height of 10-12 cm. Conical processes with terminal oscula project upwards from the lower body. With this species, which is a light-loving form, I have obtained the best results when outside aquaria, either concrete aquaria or tubs, were used. The method of treatment is briefly this: Into a tub about 60 cm. by 30 cm. and covered with glass, a half dozen sponges, freed as far as possible from live oysters and crabs, are put. They are raised from the bottom on bricks. The tub is emptied, filled and flushed for some minutes three times in every twenty-four hours. Direct rays of the sun should be avoided. Tubs answer as well as concrete aquaria, and have the advantage of being movable.

In a day or two the oscula of the sponge disappear, and the surface begins to acquire a peculiar smooth, dense and uniform appearance. Microscopic study reveals the fact that not only the oscula, but the pores also, for the most part, close, and the canal system becomes interrupted and in some degree suppressed. The mesenchyme is more uniform, and is denser than in the normal sponge, owing in part at least to the disappearance of the extensive collenchymatous (very watery mesenchyme) tracts of the latter.

The whole sponge may pass into this state and remain without great change for weeks. During this period it shrinks greatly in size, in a given case to one quarter the original bulk. The arrangement of the skeletal spicules becomes much simplified. With the shrinkage in size the sponge becomes more solid, *i. e.*, more of the canal space is suppressed. Some flagellated chambers persist and there are a few small scattered apertures on the surface. The bulk of the chambers disappear as such, the collar-cells transforming into simple polyhedral masses which become scattered singly or in groups in the general mesenchyme. The mesenchyme is a syncytium composed of well-marked cells that are



freely interconnected. The sponge in this condition closely resembles *Spongilla* in its winter phase, as described by Weltner.<sup>2</sup> Presumably water continues to circulate through the body, but the current must be an exceedingly feeble and irregular one.

As a sponge in this condition continues to shrink, it may subdivide and thus a large sponge may eventually be represented by numerous masses, in a given case about 1 cm. in diameter. Now if the sponge in this condition or if one of the masses into which it has split up, be attached to wire gauze and suspended in a live box floating at the surface of the open water of the harbor, the sponge or piece will in a few days grow and redevelop the pores and oscula, flagellated chambers, tissue differentiation, and skeletal arrangement of the normal sponge. Whether in this regeneration the transformed and separated collar cells again unite to form the flagellated chambers, I can not say. I think it very doubtful.

In the two classes of cases just described the sponge as a whole degenerates and slowly shrinks. Cellular death takes place so gradually that at no time is there any obvious corpse tissue or skeletal debris. Much more common and of far greater interest are the following cases. In these a large part of the sponge body dies in the course of two or three weeks, leaving the skeletal network still in place and bearing the brown decaying remnants of the flesh, which, as maceration continues, are washed away. In places, however, the sponge body does not die. Here masses of living tissue are left, conspicuous amidst the dead remains by their bright color and smooth, clean surface. These living fragments may be classified into three groups. First, the upper end of an ascending lobe or a considerable part of the body of the lobe may be left alive in its entirety, thus forming a more or less cylindrical mass up to 5 mm. diameter, with a length sometimes two or three times the thickness. The histological condition of these masses is not very different from that of the sponges already described. Such

<sup>2</sup> 'Spongillidenstudien, II. Archiv für Naturgeschichte,' 1893.

a mass may be said to consist of anastomosing trabeculae, separated by the remains of the canal system. The mesenchyme composing the trabeculae consists of discrete cells interconnected by processes to form a syncytium. The flagellated chambers as such have nearly disappeared, although remnants may still be recognized. In them the collar cells have transformed into simple polyhedral bodies that are widely separated. The bulk of the chambers have broken up into their constituent cells, and these are now scattered as elementary parts of the general mesenchyme. When such masses are attached to wire gauze and hung in a floating live-box they transform into perfect sponges.

A second class of surviving remnants includes masses scattered over the general surface of the sponge. These may be spheroidal and small, less than one millimeter in diameter. Usually they are flattened and of an irregular shape with lobes, suggesting a lobose rhizopod or myxomycete plasmodium. Such masses which may be connected by slender strands are commonly from two to five millimeters in the longest direction. The third class of remnants are found scattered through the body of the dead and macerated sponge, in which they sometimes occupy positions that are obviously favorable for respiration. These bodies are more or less spheroidal and small, their diameter varying commonly from one half to one and a half millimeters. In the most successful cases of treatment, the small masses, internal and superficial, are exceedingly abundant, and the dead and macerated sponge body with its contained nodules of conspicuous living tissue strongly suggests a *Spongilla* full of gemmules.

These living remnants of the sponge (bodies of the second and third classes) execute slow amoeboid changes of shape and position, behaving thus like plasmodia, and they may be designated as plasmodial masses. Microscopic examination shows them to be of an exceedingly simple character, without canal spaces or flagellated chambers. The mass does not consist of discrete cells, but is an aggregation of syncytial protoplasm studded with nuclei. The protoplasm is stored with minute



inclusions and is reticulate in arrangement. The nuclei are practically all alike, and there are no signs of persisting collar-cells. Such a mass represents a portion of the original sponge in which the degenerative changes have progressed farther than in the larger remnants. In the latter we find a syncytium made up of discrete cells among which some persisting collar-cells are distinguishable. But in the plasmodial mass the cells have united so intimately that cell outlines have been wiped out, and recognizable collar-cells (or their nuclei) have disappeared. The optical evidence points to the conclusion that the latter help to form the general syncytium, undergoing regressive changes in their differentiation which result in their becoming indifferent parts of this unspecialized tissue.

The plasmodial masses remain alive in the laboratory indefinitely, but do not transform. They attach to the bottom of the vessel, but so feebly as to be easily shaken loose. In order to see if they would transform when returned to natural conditions, I devised the simple plan of enclosing them in fine bolting-cloth bags which were hung in a live-box floating in the harbor. The bags, rectangular, were divided into compartments about an inch square with the two flat sides nearly touching. In each such space an isolated plasmodial mass was inserted, and the bag sewed up. It was found that in such bags the masses were held in place long enough for them firmly to attach to the bolting cloth. Once attached to the cloth they grow, sometimes quite through the wall of the bag to the outer water, and transform into perfect sponges with osculum, canals, pores and flagellated chambers in such abundance as to be crowded.

This ability to undergo—when the environment is unfavorable but not excessively so, regressive changes of differentiation resulting in the production of a simpler, more uniform tissue, is something that is plainly useful, *i. e.*, adaptive. In the simplified state the sponge protoplasm withstands conditions fatal to such parts of the body as do not succeed in passing into this state, and on the return of normal conditions again develops the characteristic structure and habits of the species. That this

power is exercised in nature there can scarcely be a doubt, since the conditions that are present in an aquarium must now and then occur in tidepools.

It is probable that the power thus to degenerate with production of masses of regenerative tissue is general among sponges. I first discovered the phenomenon in *Microciona*, a very different form from *Stylotella* and one in which the skeleton includes much horny matter. And in two other Beaufort species I have succeeded in producing the plasmodial masses. There is every reason for believing that the commercial sponge shares in this ability. If this is so, we have here a means of propagation which with a further development of methods may at some time become economically practicable. In any case it is now possible to study the differentiation of a quite unspecialized tissue, one that is physiologically embryonic, into a perfect sponge at any time of the year irrespective of the breeding season. We may even exercise some direct control over the size of the plasmodial masses, as the following experiment shows.

*Microciona* was kept in aquaria until the degenerative process had begun. Pieces were then teased with needles in a watch glass of sea water in such a way as to liberate quantities of cells and small irregular cell-agglomerates. These were gently forced with pipette to the center of the watch glass. Fusion of cells and masses, with amoeboid phenomena, began at once, and in half an hour quite large irregular masses existed. In the course of a few hours the masses grew enormously through continued fusion. From this time on they adhered firmly to the glass, retaining irregular plasmodium-like shapes, and the growth was inconspicuous. To bring them together once more and induce further fusion they were on the following day forcibly freed, with pipette and needle, and to clean them of cellular debris and bacteria were transferred to a tumbler (covered with bolting cloth) in which they were kept actively moving under a fine glass faucet for about thirty minutes. In the course of this violent agitation a good many masses were lost. Those remaining in the tumbler became in the next few hours notice-



ably rounder and smoother at the surface. From this experiment eighteen more or less spheroidal masses were obtained, some of which measured one half millimeter in diameter. They were similar to the small plasmodial masses produced in this species (and in *Stylotella*) when the sponges are allowed to remain quietly in aquaria. As already stated, it is only in *Stylotella* that I have directly proved the regenerative power of these masses.

Maas has just announced<sup>1</sup> that calcareous sponges (*Sycons*) when exposed to sea water deprived of its calcium undergo marked degenerative changes, which may be of such a character that the living tissue quite separates from the skeleton and breaks up into compact cords of cells showing active amœboid phenomena. The cords further constrict into rounded masses the likeness of which to gemmules is pointed out. Maas states that he is not yet in a position to say whether these masses have the power to transform into sponges, but adds that some of his observations induce him to believe that this is possible.

It is evident that Maas, working on very different forms, has independently met with the same degenerative-regenerative phenomena as are described in this communication, the essential facts of which were presented (together with an exhibit of gemmule-like degeneration masses and young sponges into which such masses had transformed) at the recent December meeting of the American Society of Zoologists. I may add that more than two years ago at the end of the summer of 1904, in my official report (unpublished since the research was still in progress) to the Bureau of Fisheries on the investigation under my charge, I described the degenerative phenomena in *Microciona* and *Stylotella*, i. e., the formation under certain conditions of confinement of minute masses presenting a likeness to gemmules, and emphasized the

<sup>1</sup>Ueber die Einwirkung karbonatfreier und kalkfreier Salzlösungen auf erwachsene Kalkschwämme und auf Entwicklungsstadien derselben. Archiv für Entwicklungsmechanik der Organismen, Bd. XXII., Heft 4, December, 1906.

probability that these masses were able to regenerate the sponge. It was not, however, until the summer of 1906 that I was able to demonstrate the truth of this view.

H. V. WILSON

UNIVERSITY OF NORTH CAROLINA,  
CHAPEL HILL, N. C.,  
February 16, 1907

#### SCIENTIFIC NOTES AND NEWS

THE honorary freedom of the City of London is to be conferred on Lord Lister.

THE gold medal of the Linnean Society, London, has been awarded to Dr. Melchior Treub, director of the Botanical Garden at Buitenzorg.

A COMMITTEE has been appointed to arrange for the presentation to the Medical Department of the University of Pennsylvania of a portrait of Dr. John Guitaras of Havana, formerly professor of pathology at the University of Pennsylvania. The portrait will be painted by Mr. Armando Menocal of Havana.

DR. W. J. MCGEE has been elected secretary of the Inland Waterways Commission, recently appointed by President Roosevelt.

PROFESSOR ELIJAH P. HARRIS, A.B. (Amherst, '55), Ph.D. (Göttingen, '59), since 1868 professor of chemistry at Amherst College, has retired from active service.

PROFESSOR ERNEST RUTHERFORD, whose call from McGill University to the University of Manchester was announced some time since, has now gone to Manchester.

DR. J. HALM, assistant at the Royal Observatory, Edinburgh, has been appointed first assistant at the Cape Observatory, in succession to Mr. S. S. Hough, F.R.S., who was recently promoted to succeed Sir David Gill as H.M. Astronomer at the Cape.

THE Chicago Chapter of the Sigma Xi Society has held three meetings during the year 1906-7. The following papers were read:

December 3, 1906—'Some Glimpses of Mexican Vegetation,' by Professor C. R. Barnes, of the University of Chicago.

February 21, 1907—'The Conduct of Research,' by Professor H. H. Donaldson, of Wistar Institute, Philadelphia.



May 11, 1907—'Some Problems in the Study of Nutrition,' by Professor R. H. Chittenden, director of Sheffield Scientific School, Yale University.

Thirty members have been elected to membership in the society during the year. At the meeting on May 11 the following officers were elected:

*President*—Professor J. P. Iddings.

*Vice-president*—Professor J. R. Angell.

*Recording Secretary and Treasurer*—Professor Carl Kinsley.

*Corresponding Secretary*—Professor F. R. Moulton.

*National Councilor*—Professor S. W. Williston.

*Members of the Electoral Board*—Professors S. W. Williston, E. H. Moore, R. A. Millikan, R. R. Bensley, J. Stieglitz.

THE Davenport Academy of Sciences has finished its annual popular scientific lecture course, the lectures this year being by Professor Reuben G. Thwaites, of the Wisconsin Historical Society; Arthur Farwell, of Newton Center, Mass.; Frank M. Chapman, of the American Museum of Natural History, New York; Professor Arthur Fairbanks, of the University of Michigan; Professor Samuel Calvin, of the University of Iowa; Professor Thomas H. Macbride, of the University of Iowa, and Professor Frederick Starr, of the University of Chicago.

PROFESSOR R. H. CHITTENDEN, director of the Sheffield Scientific School, Yale University, has accepted an invitation to deliver the Sigma Xi lectures before the Universities of Nebraska, Kansas, Iowa and Missouri next winter. These four universities have joined together to obtain a man who is an authority on some scientific subject to come west and give a joint lecture course, delivering one lecture before each university. Dr. Chittenden will give the course during the latter part of February, 1908.

THE Robert Boyle lecture of Oxford University for 1907 was delivered by Professor Karl Pearson, on 'The Scope and Importance to the State of National Eugenics,' on May 17.

DR. DAVID P. BARROWS has completed his courses of lectures, on the 'Peoples of the Philippines and the Ethnology of Malaysia,'

at the University of California. These are probably the first regular courses of instruction given at any university in America on the ethnology of these regions. Dr. Barrows sails on July 5 to resume his work as director of education of the Philippine Islands.

THE committee of one hundred, appointed by the American Association for the Advancement of Science to further the promotion, of national interest in health, met in New York City, April 18, and organized by the adoption of rules, the election of officers and the appointment of an executive committee. Professor Irving Fisher, of New Haven, presided as the temporary chairman and was subsequently elected president. Ten vice-presidents were elected, as follows: President Charles W. Eliot, Harvard University; Felix Adler, New York; Dr. William H. Welch, Baltimore; Rev. Lyman Abbott, New York; President James B. Angell, University of Michigan; Miss Jane Addams, Chicago; Hon. Joseph H. Choate, New York; Rt.-Rev. John Ireland, St. Paul; Hon. Ben. B. Lindsey, Denver; Hon. John D. Long, Boston. Mr. Champe S. Andrews was elected secretary. Professor Irving Fisher, Dr. Thomas Darlington, Professor J. P. Norton, Dr. John S. Fulton, Dr. Richard C. Newton, Professor F. F. Westbrook and Champe S. Andrews were elected an executive committee.

WE learn from the *British Medical Journal* that it is proposed to found a prize in memory of the late Dr. Paul Julius Möbius of Leipzig, well known for his contributions to the literature of neurology and psychiatry and by his medico-literary studies on Goethe and other celebrities. The prize is to be known by his name, and is to be awarded every year for the best essay dealing with some neurological or psychiatric subject. A committee, which includes among its members Professor Edinger, of Frankfurt-on-the-Main; Professor Moeli, of Berlin, and Dr. Lamhofer, of Leipzig, has been formed to collect subscriptions for the purpose.

PROFESSOR HENRY CREW, Fayerweather professor of physics in Northwestern University, has been elected president of the Northwestern Chapter of Sigma Xi. On May 24 Professor



William A. Locy delivered before this chapter a lecture on 'The Life and Work of Linnæus.'

THE Zoological Club of the University of Nebraska celebrated the centenary of the birth of Louis Agassiz on May 28. Addresses were given by Professor H. B. Ward on 'The Debt of the New World to Louis Agassiz,' and by Professor F. D. Losey on 'Lowell's Tribute to Agassiz.'

THE Denison Scientific Association on May 28 celebrated the one-hundredth anniversary of Agassiz's birth by a memorial exercise at which addresses were made by Dr. E. W. Hunt, president of Denison University, on 'The Personality of Agassiz'; by Professor M. E. Stickney, on 'Penikese and the Agassiz Museum'; by Professor Frank Carney, on 'Agassiz's Contribution to Geology,' and by Professor C. J. Herrick, on 'Louis Agassiz and the New Natural History.' These addresses were delivered in the new Barney Memorial Hall of Science of Denison University and were followed by a reception at which the building was opened to the public for inspection. This hall was originally built in 1894 and was destroyed by fire in 1905. It has been rebuilt fire-proof and greatly improved in other respects by the original donor, Mr. E. J. Barney, of Dayton, O.

A TABLET to the memory of Dr. Mary Putnam Jacobi was unveiled in the Woman's Medical College, New York, May 23. An address was made by Dr. Bertha Lewis on behalf of the alumnae association, and Dr. William Welch accepted the tablet. The memorial tablet is of brass and is inscribed as follows. "In memoriam, Mary Putnam Jacobi, class of 1864, Woman's Medical College of Pennsylvania; president of the Alumnae Association 1881-1891 and 1894-1895. Ecole de Médecine, Paris, class of 1871; professor of materia medica and therapeutics, Woman's Medical College of the New York Infirmary; professor of the diseases of children, New York Post-Graduate Medical College and Hospital; fellow of the New York Academy of Medicine. A distinguished contributor to medical literature, and one of the most eminent women of her time in the medical profession."

At the recent session of the Pennsylvania legislature the senate voted \$300,000 to enable the American Philosophical Society to erect a memorial to Franklin, but the house did not concur in the bill.

THE death is announced of Dr. Charles Féré, physician to the Bicêtre, Paris, well known for his researches in neurology and psychiatry.

SIR JOSEPH FAYRER, well known for his pathological work in India and as one of the most prominent British physicians, died at Falmouth on May 21, at the age of eighty-three years.

MR. SAMUEL LORD MORRISON, A.B. (Harvard, 1873), an engineer, known especially for his work on filtration plants, died in London on May 21, at the age of fifty-six years.

DR. VOLZ, of Berne, while making natural history collections in the Liberian Hinterland, was murdered by natives.

DR. WILHELM MÜLLER, curator of the mineralogical collections of the Technical Institute of Berlin, died on May 2.

THE death is announced at the age of fifty-two years of M. Charrin, professor of general and comparative pathology at the Collège de France.

A COMMITTEE of Filipinos appointed to investigate the inoculation of prisoners at Bilibid Prison with contaminated serum, has made a report. The committee finds that no one was responsible for the accident, and exonerates Dr. F. P. Strong, of the Bureau of Science, who was in charge of the bacteriological work, and was conducting the experiments.

A CONFERENCE of state geologists was held with the officers of the National Geological Survey at Washington, on May 20, to discuss cooperative work in various states by the federal and state authorities. State geologists were present from Alabama, Connecticut, Illinois, Indiana, Iowa, Kentucky, Maine, Maryland, Michigan, Missouri, New Jersey, New York, North Carolina, Ohio, Pennsylvania and South Carolina.

THE Paris Society of Chemistry celebrated the fiftieth anniversary of its foundation on



May 17; among the guests were Professor W. H. Perkin and Professor Henry E. Armstrong.

THE SWISS Scientific Society will hold its ninetieth annual meeting at Fribourg, beginning on January 28. The Swiss National societies of geology, botany and chemistry will meet at the same time as the sections of the general society.

MR. W. T. HORNADAY has presented to the New York Zoological Society his collections of heads, horns and tusks, comprising 131 specimens, representing 108 species. These are to form the nucleus of a collection to be exhibited at the New York Zoological Park, and it is hoped notable additions may promptly be made. Mr. Charles T. Barney has recently given the world's record tusks of African elephant, one measuring 11 feet in length, the other 11 feet 5½ inches. The weight of the pair is 293 pounds. The record pair as regards weight is that shown by Tiffany & Co. some years ago, whose combined weight was 463 pounds.

THROUGH the generosity of Messrs. Alfred F., Charles C., and John S. Pillsbury, of Minneapolis, Dr. Thomas G. Lee has secured for the department of histology and embryology, University of Minnesota, the *Handapparat*, or working library, of the late Professor William His, of Leipzig. This collection comprises over 8,400 monographs and separates contributed by over 2,500 different authors.

THE *Journal* of the American Medical Association states that the library of the Royal College of Surgeons, which is the largest medical library in Great Britain and the largest in Europe, with the exception of the library in Paris, has outgrown the space allotted to it. It now contains 100,000 publications, made up of 60,000 volumes and 40,000 pamphlets. It increases at the rate of 1,000 volumes a year. A large room has been added over the library, which in former years consisted of a suite of apartments occupied by a clerk who lived in the college, a custom no longer followed. This room will be principally devoted to work on public health and official government reports. Thus space has been obtained for another 10,000 volumes.

MR. WALTER WELLMAN has now left for Norway en route for Spitzbergen on his second year's work in connection with his project for reaching the North Pole by means of his airship *America*, which is now on its way to Norway. In a statement made to Reuter's representative, he said all the members of the expedition would meet at Tromso, from which place they would sail on June 1 on board the expedition steamer *Frithjof* for Spitzbergen, where they will arrive on June 5 or 6. The rest of that month will be occupied in installing gas apparatus, enclosing the great balloon house, and assembling the car, motor, etc. At the end of June the balloon will be inflated. In the first week of July there will be trials of the airship until it is demonstrated that it is ready for the voyage. The start for the Pole will be made on the first favorable opportunity afterwards, probably between July 20 and August 10, but, if necessary, Mr. Wellman is prepared to start as late as August 20. The members of the expedition are: Mr. Walter Wellman; Major Hearsey, executive officer and scientific observer, who has been lent by the United States Government; Chief Engineer Vannerman, who is already in Norway; Dr. Fowler, surgeon; and M. Gaston Hervieu, the aeronautical engineer. About thirty men, sailors, mechanics, &c., will be embarked at Tromso, making a total expeditionary force of forty men, including Mr. Felix Riesenbergh, the navigator of the expedition, who, together with two Norwegian companions, has spent all the winter at the base at Spitzbergen.

THE lecture arrangements at the Royal Institute of Public Health for the summer session are as follows: The Harben Lectures will be delivered by Professor Paul Ehrlich, director of the Royal Institute of Experimental Therapeutics at Frankfort, on June 5, 7 and 11, the subject being experimental researches on specific therapeutics. Lectures will be delivered each Thursday from May 23 to June 20, both days inclusive, on the Veterinary Aspects of the Tuberculosis Problem, by Professor J. Penberthy; on the Problem of a Pure Milk Supply, by Professor R. T. Hewlett; on Blood Immunity, by Professor G.



Sims Woodhead; on the Treatment of Infectious Diseases regarded from the point of view of hospital administration, by Dr. E. W. Goodall, Medical Superintendent of the Homerton Fever Hospital; and on the Development of Africa, as a problem of comparative pathology, by Dr. L. W. Sambon. The lectures are all free.

THE Botanical Department of the University of Pennsylvania will hold its closing meeting and reception in the Botanical Garden on June 8, from five to ten in the evening. At 7 P.M. Provost Charles Custis Harrison, LL.D., honorary president of the society, will make the introductory address, followed by a series of lectures and short talks and an inspection of flowers and specimens, many of them added to the department since the last annual meeting by gift or in consequence of the travels and researches of members of the faculty and students.

*Nature* gives the following account of the program of the meeting of the International Union for Cooperation in Solar Research, which was held recently in Meudon, near Paris: "The meeting will open on May 20, when formal business will be transacted in the morning. In the afternoon it is intended that all new proposals for joint work shall be submitted to the meeting, so that members will have an opportunity of privately discussing the desirability of adopting the proposals before a final decision is taken towards the end of the week. The mornings of May 21 and 22 will be spent in receiving the reports of the committees appointed at the Oxford meeting in 1905. It is understood that Professor Pérot is ready to submit his measurements of the wave-length of the red cadmium line, and that his results are in such good agreement with those previously obtained by Michelson that the meeting probably will be able to adopt finally a primary standard of wave-length. Other reports deal with the observations of sun-spot spectra and the organization of the systematic application of the spectroheliograph to solar work. A question of interest to which several members of the union have given considerable attention con-

sists in fixing the best methods of measuring the areas of flocculi. This matter has been under consideration at some of the American observatories, as well as at the Solar Physics Observatory at South Kensington and at the University Observatory, Oxford. On Tuesday evening Dr. Janssen, the president of the congress, will give a banquet to the members at the Hôtel d'Orsay, in Paris, and on Wednesday afternoon Professor Julius will demonstrate in the physical laboratories of the Sorbonne some of his experiments on anomalous dispersion. Arrangements have also been made to visit the Observatory of Paris in the same afternoon. It is hoped that the scientific work of the meeting will be concluded on May 23, and an excursion to the Château de Chantilly has been arranged for Friday. A formal business meeting on May 25 will bring the meeting to a close."

#### UNIVERSITY AND EDUCATIONAL NEWS

THE Pennsylvania legislature voted at its recent session an appropriation of \$500,000 for the University of Pennsylvania. The bill has not yet been signed by the governor.

MR. AND MRS. JOHN C. HEMMETER have given an endowment for the chair of physiology of the University of Maryland.

AN alumnus of Hobart College has given \$20,000 for a new gymnasium.

ONE of the dormitories of Trinity College, Hartford, Conn., was injured by fire on May 22. Some damage was also done to the library, the entire loss being estimated at \$15,000.

SARATOFF has been chosen as the seat of the new Russian University which is to replace that of Warsaw.

THE University of Montana announces the establishment of fellowships in the departments of chemistry, botany, physics and mechanical engineering, each with an income of five hundred dollars annually in addition to tuition and laboratory fees. Each fellow will be expected to devote approximately half his time to assistance in the department in which he is chosen. It is desirable that the fellow



in botany should be interested in forestry, and the fellow in chemistry will be expected to take charge of the laboratory work in the first year's work in inorganic chemistry. There is an excellent opportunity for research work on smelter problems, and also along some lines of electro-chemistry. Graduates of any reputable college or university are eligible. Applicants should send their credentials to Dr. O. J. Craig, president, Missoula, Montana.

MR. DANIEL W. O'HERN, A.B. (Drake), A.M. (Virginia), now of the Johns Hopkins University, has been appointed associate in geology in Bryn Mawr College.

At Amherst College, Professor W. J. Newlin, associate professor of mathematics and psychology, has been appointed associate professor of philosophy. He will continue next year the work he has carried on since the death of Professor Garman.

THE following changes have been made in the faculty and instructing staff of the Massachusetts Institute of Technology: Promotions from associate professor to professor: John O. Sumner, A.B., professor of history; Frederick H. Bailey, A.M., professor of mathematics; Henry Fay, Ph.D., professor of analytical chemistry. New appointment: Reginald A. Daly, Ph.D., professor of physical geology; Professor William O. Crosby, S.B., has been retired under the Carnegie Foundation. Promotions from assistant professor to associate professor: Henry G. Pearson, A.B., associate professor of English; Ralph R. Lawrence, S.B., associate professor of electrical engineering; George C. Shadd, S.B., E.E., associate professor of electrical engineering. New appointment: Edwin B. Wilson, Ph.D., associate professor of mathematics. Promotions from instructor to assistant professor: Leonard M. Passano, A.B., assistant professor of mathematics; George L. Hosmer, assistant professor of civil engineering; Charles B. Breed, S.B., assistant professor of civil engineering; Maurice De K. Thompson, Ph.D., assistant professor of electro-chemistry; Henry L. Seaver, A.B., assistant professor of English. New appointments: Gilbert N. Lewis, Ph.D., assistant professor of physico-

chemical research; Earle B. Phelps, S.B., assistant professor of research in chemical biology. The following instructors have received leave of absence: Clifford M. Swan, S.B., instructor in physics; Clarence L. E. Moore, Ph.D., instructor in mathematics; Francis Harold Dike, A.B., instructor in modern languages. Return from leave of absence: Daniel F. Comstock, Ph.D., instructor in theoretical physics. Resignations: Raymond Haskell, S.B., S.M., instructor in theoretical chemistry; Champion H. Mathewson, Ph.D., instructor in analytical chemistry. Promotions from assistant to instructor: Clinton H. Colleser, A.M., instructor in English; Harold G. Crane, S.B., instructor in electrical engineering; George A. Rodenbaeck, S.B., instructor in electrical engineering. New appointments: Nels J. Lennes, M.Sc., instructor in mathematics; Richard C. Tolman, S.B., instructor in theoretical chemistry; Robert S. Williams, instructor in analytical chemistry; Henry B. Phillips, Ph.D., instructor in mathematics. Appointments as assistants: Charles R. Bragdon, A.B., S.B., assistant in theoretical chemistry; Paul S. Fiske, A.B., assistant in inorganic chemistry; George F. White, S.B., assistant in organic chemistry; Frank B. Shields, assistant in technical analysis; Herman W. Mahr, research assistant in organic chemistry. Resignations: John C. Hudgins, A.B., assistant in inorganic chemistry; Ralph S. Gifford, S.B., assistant in theoretical chemistry; Frank J. Quinlan, assistant in inorganic chemistry; Albert H. Smith, assistant in mechanical engineering; Albert L. Smith, S.B., assistant in analytical chemistry; Anna M. Cederholm, S.B., assistant in technical chemical research; Walter G. de Steiguer, S.B., assistant in geology; Arthur Neale, S.B., A.R.C.Sc., assistant in technical analysis; Fred C. Mabee, A.M., research assistant in physical chemistry; Ledyard Sargent, A.M., research assistant in physical chemistry; E. B. Spear, B.A., research assistant in physical chemistry. Lecturers — New appointments: James F. Kemp, A.B., E.M., Sc.D., on economic geology; M. C. Whitaker, S.M., on factory organization and management.